NATIONAL
ACADEMY OF SCIENCES
OF THE UNITED STATES
OF AMERICA

BIOGRAPHICAL MEMOIRS

VOL. XVI

CITY OF WASHINGTON
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James Furman Kemp -

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BIOGRAPHICAL MEMOIRS
VOLUME XVI—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

JAMES FURMAN KEMP

1859-1926

BY

FRANK DAWSON ADAMS

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1933

JAMES FURMAN KEMP

August 14, 1859—November 17, 1926

BY FRANK DAWSON ADAMS

James Furman Kemp was born in the City of New York on August 14, 1859, and passed away at Great Neck, Long Island, N. Y., on November 17, 1926, in the sixty-seventh year of his age. He was stricken down suddenly and without any previous warning as he was about to enter the train leaving Great Neck, where he resided, for New York, to meet his morning classes at Columbia University. Only thirty-six hours previously he had been the speaker at a largely attended meeting of the New York Section of the American Institute of Mining and Metallurgical Engineers, where he described certain mines in Spain and Portugal which he had visited during the preceding summer when attending the 14th International Geological Congress which met at Madrid in 1926, to which he had been appointed an official delegate by the President of the United States, and at which he also represented several important scientific societies.

His great grandfather, Joseph Alexander Kemp, came to America from Perth in Scotland in 1797, and settled in Albany, where he married Elizabeth Jillson. Later this great grandfather left Albany and took up his residence at Newburgh-on-Hudson. Professor Kemp's father, James Alexander Kemp, was born in the City of New York in the year 1831 and became a partner in a firm of wholesale grocers in that city. He married Caroline Anna Furman and they had three children, of whom James Furman Kemp was the youngest, the two other children having died before his birth. While he was yet a young boy his parents moved to Brooklyn, N. Y., where they resided during the remainder of their lives.

When this boy James, who was destined to attain marked distinction in the world of science reached school age, he was sent by his parents to Lockwood's Academy in Brooklyn and later to Adelphi Academy, now known as Adelphi College, in the same city, from which he graduated in 1876. From the first he took a keen interest in natural history and was an industrious

collector, especially in the field of botany. From Adelphi College he went to Amherst, from which he graduated as Bachelor of Arts in 1881. To Amherst about this time came a notably large number of men who subsequently attained distinction in one branch or another of the geological sciences. Among these were George H. Williams, W. B. Clarke, Whitman Cross, John M. Clark and others, all of whom came under the inspiring influence of that excellent teacher, Professor B. K. Emerson, a man having a wide knowledge of many subjects as well as a thorough mastery of his own. Emerson's infectious enthusiasm and his magnetic personality were important factors in determining the course and current of the lives of these men, and of Kemp's among them. After leaving Amherst he entered the Columbia School of Mines where he followed a course of more technical training, graduating in the year 1884 with the degree of Engineer of Mines. Once again, at Columbia, he had the good fortune to come under the influence of an inspiring teacher, Dr. John Strong Newberry, the distinguished geologist and palaeontologist, and it was through Newberry's influence that his attention was directed definitely to the study of geology as his life's work.

He then took the wise course of going abroad to continue his studies and thus see the geology of other countries and the problems of geology from other viewpoints than those of his nati e land, excellent as these may be. He made a wise selection in choosing the German universities and spent the next two years at Leipzig and Munich, attending more especially the lectures of Von Zittel and Von Groth at the last mentioned university. He did not, however, proceed to a degree at either of these seats of learning.

F. G. Corning in his interesting little book on early student days at the Freiberg Royal Mining Academy, says that Professor Kemp was also at Freiberg and although not enrolled as a regular student there he visited the Academy and took part in the field excursions.

While in Germany he became acquainted with Henry Shaler Williams, Professor of Geology in Cornell University, who was

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impressed by his character and ability. When Williams returned to Ithaca he requested the authorities of Cornell University to establish an assistantship in the department of geology and allow him to select the person who should be appointed to fill this position. The Trustees granted this request and Professor Williams selected young Kemp for the place. Kemp put his foot on the first rung of the ladder of success in this appointment to a junior position on the staff of the department of geology at Cornell University in 1886. He eventually rose to the position of assistant professor at Cornell, where he remained until 1891. In this year he received a call from Columbia University to the position of adjunct professor of geology under his former teacher, Professor Newberry. He accepted this and upon the death of Professor Newberry in the following year he became head of the department of geology, which position he continued to occupy until the time of his death.

His home life as a growing boy and a young man was not altogether a happy one. His parents were unsympathetic with his ambition to enter college and fit himself for a professional career, and it was only with great reluctance that they provided him with the scanty funds which, with such additions as he could himself secure by engaging in work during his vacations, would enable him to meet his college and university expenses.

It was therefore with especial pleasure that he received his first university appointment which provided him with adequate means for his support and thus enabled him to devote his undivided time to the work of his chosen profession, without an ever present consideration of the question of ways and means.

In 1889, during his professorship at Cornell University, he married Kate Taylor, daughter of John Nichols Taylor of Kingston, R. I. It was a very happy marriage and throughout the long course of their married life Mrs. Kemp gave to her husband such continuous and invaluable support and encouragement as to contribute in no small measure to the success which he achieved. Had it not been for her unremitting care during his long illness in 1915 and 1916 it is indeed doubtful whether he would ever have survived. To this marriage three children were

born, two sons—James Taylor Kemp, now technical adviser to the American Brass Company at Ansonia, Conn., and Philip Kitteridge Kemp, rector of St. Marks Episcopal Church at Glendale, Cal., and one daughter, Katherine Furman Kemp, who married Chase Donaldson, Esq., and who died in 1929.

Professor Kemp loved teaching and pursued his work at the University with a wholehearted enthusiasm which he communicated also to his students. He had, moreover, the power of presenting in a most lucid manner and in language which was readily understood, even the most difficult subjects with which he had to deal. His students were in consequence always deeply interested in their work and this interest was felt by Dr. Kemp to be a rich reward for all the care and trouble which the preparation for his work entailed.

At Columbia his university work, as years went on, became very heavy and exacting, for not only did the number of his undergraduate students increase rapidly from a few tens to several hundred, but the number of post-graduate students going forward to the degree of Doctor of Philosophy in geology and the cognate sciences, also grew rapidly, and such students working on difficult and advanced problems often in widely separated fields required an immense amount of individual attention and assistance. But his labors were not confined to the work of his department, for as his abilities, and what may be justly called his wisdom, became known to his university colleagues and to the officers of the many scientific societies with which he became associated, he was called upon to serve on a multitude of boards and committees, and was also chosen to fill many important executive positions. He was always interested in athletics and among the university committees of which he was a member at Columbia was that of the committee on athletics, the work in connection with which he found especially trying, presenting as it did year after year the everlasting problem of attempting to harmonize the ever increasing demands of competitive athletics with academic ideals. From its foundation he also served as a member of the board of managers and as a

scientific director of the New York Botanical Gardens. He was a member of the Columbia University Club, the Amherst Club, and of the Century Association of New York. He also took a very keen and active interest in many of the great scientific societies. He was one of the group of thirteen men who in the year 1888 organized the Geological Society of America. He was a charter member of this Society and was its secretary for many years and, having served in almost every capacity in it, was elected as its president in 1921. He was president of the New York Academy of Science, and of the Society of Economic Geologists, and vice president of the American Association for the Advancement of Science. In 1912 he was elected president of the Mining and Metallurgical Society of America and received its gold medal in 1914 and was made an honorary member in 1917. He became a member of the American Institute of Mining and Metallurgical Engineers in 1891 and was on its board of management from 1896 to 1898. He was vice president of the society in 1903 and 1904 and president in 1912. He was also a member of the American Philosophical Society and of the American Association of Petroleum Geologists. In 1911 he was awarded the blue ribbon of scientific attainment in the United States by being elected to membership in the National Academy of Sciences.

In addition to these he was elected a corresponding member of the Geological Society of London, the Geological Society of Stockholm, the Geological Society of Belgium, the Academy of Oslo, and the Canadian Institute of Mining and Metallurgy. His Alma Mater, Amherst, in 1906, conferred upon him the degrees of D.Sc. Honoris Causa, and in 1913 on the occasion of the meeting of the Twelfth International Geological Congress in Canada, McGill University awarded to him the honorary degree of LL.D.

He was an excellent after-dinner speaker and spoke very frequently at the banquets given by the various societies with which he was connected. For many years his former teacher, Professor B. K. Emerson of Amherst, was toast-master at the

annual banquet of the Geological Society of America and his brilliant talks on these occasions will long be remembered by the members of the Society who had the pleasure of being present on these occasions. When in the course of time he was no longer able to attend the meetings of the Society, Professor Kemp succeeded him and discharged the duties of this rather difficult position with admirable grace and remarkable ability.

Professor Kemp was a prolific writer. His work entitled "Ore Deposits of the United States" was published in 1893 and passed through many subsequent editions. Three years later, in 1896, his "Handbook of Rocks" was issued. But the result of his work and investigation appeared chiefly in papers presented to various scientific societies and printed in their proceedings, or in reports to the United States Geological Survey or to the New York State Survey. During a portion of each summer for a number of years he devoted his time to the geological mapping of certain areas in the mineral bearing portion of the State of New York, for the last mentioned Survey. He was one of the founders and an associate editor of Economic Geology and many articles from his pen appeared in this journal.

At the beginning of his career he took an especial interest in mineralogy but later, although he worked over a wide field in geological science, he devoted his attention more particularly to economic geology and especially to the science of ore deposits, in which subject he became one of the leading authorities in North America. He also took an especial interest in the geology of the great areas of pre-Cambrian exposed on this continent and devoted much close study to the pegmatites which occur so abundantly in many places in these ancient rocks.

The range of his studies can best be seen by an examination of the accompanying bibliography of his published writings, which has been prepared for the writer by Professor Berkey, the able successor of Dr. Kemp at Columbia University.

His advice was also sought in connection with the development of many important engineering works, and in the development of many mining areas where difficult problems presented

themselves for solution. He acted as consulting geologist to the Anaconda Copper Company, The Calumet and Heckla Consolidated Copper Company, the Spanish American Iron Company, The Port Henry Iron Ore Company (Mineville, N. Y.), and the New Jersey Zinc Company, also in connection with the new Croton Dam and to the Board of Water Supply of New York City in the selection of a route for the Catskill Aqueduct.

His opinion was also sought in connection with many important law suits relating to mines and mineral areas. His wide knowledge of these subjects, his absolute honesty and his power of clear exposition made his testimony of great weight and value. But the great strain which these intense and manifold duties and occupations entailed upon Dr. Kemp resulted in 1915 in a complete nervous collapse and for over two years he was obliged to give up work of every kind, spending most of the time in the open air on the coast of Florida. He eventually completely recovered his health and once again entered upon his work with the same vigor and carried it on with the same intensity as before. But while his brain withstood this renewed strain, his heart could not, and, as already mentioned, he was stricken down and died almost instantly in 1926.

It was very fortunate that during his prolonged absence from Columbia University he had in Professor C. P. Berkey a very able coadjutor, who together with the other members of the geological staff at Columbia, which by this time he increased from three to ten in number, carried on his work uninterruptedly until he could resume it.

But while Professor Kemp was a man of very marked ability, wide knowledge, and widely recognized attainments, the outstanding characteristic which impressed itself at once on every one who met him, was his genial personality. Goodness and benevolence seemed to surround him like an aura and he met everyone with a display of interest which, as a matter of fact, he actually felt. Thus he made a friend of everyone he met. His students, who were naturally brought into close contact with him and who were often in especial need of help and en-

couragement, came to regard him rather as a father than a teacher and in after life, scattered as they were over every known part of the habitable world where mines exist or geological work is to be done, they retained for him a very special and peculiar affection. An excellent portrait of him, in oils, was presented to Columbia University by his former pupils, on the occasion of the celebration of the 250th anniversary of this University, and now hangs in the library of the department of geology in Schermerhorn Hall. Wherever he went engineers or mine managers, former students of his, appeared and welcomed him. A past president of the Geological Society of America in the course of a humorous address delivered at one of the annual dinners of this Society some years ago, remarked that it had been his fortune to travel through almost every part of North America and while in the course of his journeys he had visited many places which were "wild and woolly," he had never yet found one which was "un-Kempt"!

Possessing this essentially generous nature, his help was asked by many persons and freely given, as far as his time and means allowed. In the great majority of cases this was requited, but as tares are generally mingled with the wheat in the field of this world, in some cases it was not. In these as well as in other cases where his confidence had been abused, although he felt such things very deeply, he never expressed any resentment at such untoward acts but seemed to desire to sweep them from his memory as speedily as possible and pass on to other and more worthy things. It is very doubtful whether he ever used a harsh expression in his life.

And so in passing away, Professor Kemp has left a fine record of good work accomplished in the furthering of the knowledge of the science which he professed and in passing the torch of knowledge on to many younger men who are now carrying it forward imbued with his spirit. He has also left the very fragrant memory of a fine, good and noble life as an example and inspiration to everyone who knew him, each of whom will join the writer and say in the words of Hamlet, "He was a man, take him for all in all, I shall not look upon his like again."

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Raphael Pumpelly

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XVI—SECOND MEMOIR

BIOGRAPHICAL MEMOIR

OF

RAPHAEL PUMPELLY

1837—1923

BY

BAILEY WILLIS

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1931

MEMORIAL OF RAPHAEL PUMPELLY [1]

BY BAILEY WILLIS

CONTENTS

FOREWORD

Pumpelly's place is among the great explorers. He inherited the spirit; his environment gave the motive; he achieved the career by virtue of his physical endowment, his high courage, and his mental and moral fitness.

His interest in geology was first aroused by his mother, an unusually gifted woman, and was fixed by early incidental opportunities for observations in Germany and Corsica, followed by a chance meeting with the German geologist, Noeggerath, which led to his going to the mining school at Freiberg. He used his profession of mining engineer, but never devoted himself to it. He served his adopted science, geology, but not exclusively; yet his contributions to its principles and knowledge remain unchallenged after thirty to fifty years of later intensive research along the same lines. He cherished for forty years a dream of carrying out investigations in the archeology and ethnology of the Asiatic and European races and at the age of three score and ten realized it with that same thoroughness and devotion to truth which characterized all of his scientific studies.

BOYHOOD

Pumpelly's paternal grandfather fought through the French and Indian wars and the Revolution. At the close of the latter he moved

[1] Reprinted from the Bulletin of the Geological Society of America; vol. 36, 1925.

out onto the then frontier of New York, near Ithaca, with his four sons, of whom John, the oldest, had spied out the land and William, a blond, six feet two in height, became Raphael's father. William is described by his son as a man of noble features and fine presence, of equable and kindly temperament, and with broad sympathies. He transmitted his nobility of spirit, his optimism, and his kindly sympathetic nature to his son.

Raphael's mother came of families that had ruled colonial Connecticut. He says of her: She was of medium stature, erect and energetic, affectionate, and artistic in temperament. He was not trained, as she was, to be an artist, but he inherited from her an appreciation of the beautiful, which she cultivated, and it is evident from his reminiscences that she exercised a dominant influence upon his intellectual and moral development from boyhood on.

Raphael Pumpelly was thus born of English colonial stock, pioneering on the forested frontier, engaged in surveying the boundaries of new states and land grants, clearing the timberlands, exploiting the wealth of the wilderness, and building up the young Commonwealth of New York. Owego, his birthplace, was but forty-five years old when he was born; yet such had been the energy of the settlers that the forest felling, the rafting of lumber down the Susquehanna, the building of roads, and other truly pioneer activities had begun to give place to the established life of an American country town, with good schools and schoolmasters whose wholesome, though stinging, discipline Raphael remembered after threescore years and ten.

N. P. Willis, writing when Raphael was ten or twelve years old, thus described the scenes among which the boy grew up:[2]

> "At Owego there is a remarkable combination of bold scenery and habitable plain. A small, bright river comes in with its valley at right angles to the vale and stream of the Susquehanna, forming a star with three rays or a plain with three radiating valleys, or a city (in the future, perhaps) with three magnificent exits and entrances. The angle is a round mountain, some four or five hundred feet in height, which kneels fairly down at the meeting of the two streams, while another mountain of an easy acclivity lifts gracefully from the opposite bank, as if rising from the same act of homage to Nature. Below the town and above it the mountains for the first time give in to the exact shape of the river's short and capricious course: and the plain on which the town stands is enclosed between two amphitheaters of lofty hills, shaped with the regularity and even edge of a coliseum and resembling the two halves of a leaf-lined vase, struck apart by a twisted wand of silver."

[2] N. P. Willis: Rural letters, letters from under a bridge, 1849.

Of the Susquehanna Willis wrote:

"This delicious word, in the Indian tongue, describes its peculiar and constant windings; and I venture to say that on no river in the world are the grand and the beautiful in scenery so gloriously mingled. The road to Owego follows the course of the valley rather than of the river, but the silver curves are constantly in view; and from every slight elevation the majestic windings are seen, like the wanderings of a vein, gleaming through green fringes of trees and circling the bright islands which occasionally divide the waters. It is a swift river and singularly living and joyous in its expression."

We may not assume that the scenery impressed the boy as it did the poet, but the river played an important part among the youngster's early stimuli. Twice nearly drowned, he feared to swim beyond his depth; yet, fearing ridicule even more, he dared the boys to follow him on thin ice, or to shoot a dam on a plank, or (most daring of all) to navigate the river in flood on a cake of ice. This last, he records, found no takers and twice nearly finished him. He afterwards looked back on this "bravado" as a most valuable part of his training, for "it taught quickness of perception, the instant cooperation in emergencies of brain, hands, and feet, and that balance between caution and action that forms a basis of judgment."

But it was not alone as a dangerous playfellow that the river entered into the boy's life. Its swift current, coming from who knows where and flowing on into the unknown, became to his adventurous spirit a symbol of life, leading him on, unconscious of its influence, to that career of exploration for which he was so eminently fitted. He knew nothing of the lands he was to visit—Corsica, Arizona, Japan, China, or Siberia. He had no idea of the geological and ethnical problems in which he later interested himself. All that was around the bend. But the shining river, flowing swiftly westward in gleaming curves, can not but have led his fancy on, as he watched the great timber rafts float out of sight, or listened to stories of voyages beyond his ken.

He studied at the Owego Academy, perfunctorily apparently; his real school was out of doors. There he trained his faculties, very much as young Indians had done in the same environment but a few decades before him. His self-respect had not developed beyond the childish principle that one should not be found out. But a piratical downward course on which he and his youthful followers had entered was brought to a sudden close by a serious offense, and he was led to a sense of responsibility for his acts through the searching, though loving, reasoning of his mother and the sound chastisement she gave him in due sequence. Following this, when eleven years old, he was sent away to school to General

Russell's at New Haven, where he spent the next six years, developing by his own account into a manly and honorable, but daring and irresponsible, youth.

In the natural course of events Raphael would have followed the lead of a majority of the scholars of General Russell's Academy and have entered Yale; it was therefore in keeping with the scheme of his life that he went to Europe instead. The thought of so doing was first suggested by his chum, who in the end did not go, but it was the cordial interest taken by his mother in a European journey which made possible the realization of the boy's unusual proposal. It is evident that he and she were comrades in initiative, congenial in spirit and in tastes, and that she, no less than he, looked forward eagerly to the pleasures of a tour abroad. In 1854 the parting on such a journey was a very different thing from what it is today, but her eager, artistic spirit rose to the opportunity without thought of consequences, as was ever his instant impulse.

The experiences of an American boy of seventeen or eighteen as a free lance in Germany or France are the reactions of his character with his environment, as definitely fixed as are chemical combinations. Pumpelly's were no exception. Frankly interested in everything about him—languages, social ways, or scenery—he met every new impression eagerly, instinctively gathered the good from it and passed on in search of further novelty. He acquired French, German, and Italian by listening and talking rather than by study. He readily adopted the ways of congenial companions and made friends among young and old; but one experience of students' carousing disgusted him. He turned rather to his independent life, attending lectures on all kinds of subjects for what he could learn of the language and wandering far afield among the picturesque and romantic scenes of central Germany. Long afterwards he remembered that.

"During the month of May (1855), wonderfully beautiful that year, I tramped up and down the valley of the Rhine and into the interior on each side. To my young imagination that month was a delight. The valley was as yet untouched by the wand of modern industrial desecration, the air was still pure, the sky serene, and the castles crowning the hills were still real moss- and plant-clothed ruins.

"Who can describe the effect of it all on the impressionable imagination of a boy fresh from the land of modernity; the charm of the ever-varying scenery, the vineclad slopes, the echoing cliffs of the Siebengebirge, the castles, the Lorelei-haunted Drachenfels, and the romance and legends of the past?

"Then to me there was another interest which seemed to intertwine itself with the romance and poetry of the region. Here for the first time I came in contact with eruptive rocks, the striking sanadine trachyte cliffs of the

Drachenfels and the porous sapphirine-bearing lavas of the Laacher See. Truly I was entering, though gropingly, into geology through the gate of romance."

Thus Pumpelly gives us the clue to his interest in geology; but the picture of himself is not complete without the added touch of interest in his fellow-men, which was always the dominant one of the two. He continues:

"Bon Cœur (a Newfoundland dog) was always with me, sleeping on or near my bed at night and tramping alongside by day. Admired by all, he introduced his master to many; so that I never lacked chances to talk with men and women of every class, whether residents or travelers."

His attention had been attracted, when he was still a mere boy, to the fossils which are abundant in the strata of the gorges about Owego, and a teacher with whom he had wandered along the Bronx near New York had stimulated his interest in the minerals and metamorphic rocks of that region. While at Hanover he collected ammonites, crinoids, etcetera, from the Deister Mountains, and, hearing that there was a geologist in the neighboring town of Hildesheim, he made an unsolicited visit on the well-known paleontologist Von Roemer in order to see his collection. He was kindly received, and the scientist then and afterwards gave him further insight into the realm of geology and the line of relationships which link extinct forms with the living. Darwin had not then "hurled the bomb of evolution," but the seeds of that understanding were planted in Pumpelly's mind by von Roemer.

More than a year was to pass, however, before his growing interest in geology was to give a definite purpose to his education. In the meantime he continued to develop the habit of observation, with special reference to geologic phenomena and human nature. He made a summer excursion in Switzerland, where the grandeur of the mountains did not fail to make an imperishable impression on his memory. He spent part of a winter in Paris, and left there for Naples to recover from a cold which attacked his lungs. At Naples he resumed his roaming about the country to satisfy his curiosity—we had better call it, rather than interest—in the volcanic phenomena and in the romantic background of Nature and people. He wrote:

"I haunted Vesuvius. I liked to trace the destroying streams of lava that during centuries had coursed down the slopes to the sea, and I noticed as well as I could their difference in structure. Then there was the pleasure in searching for the fragments of limestone hurled out from great depths. They had been changed in the subterranean laboratory to marble, and often contained a variety of beautifully crystallized minerals peculiar to Vesuvius.

Of course, I did not know anything about these minerals, except that they were beautiful in my eyes."

He visited Solfatara, where in the extinct crater he saw the rocks undergoing decomposition by gases with the formation of new minerals. He realized that rocks and minerals are not dead.

"Their stories were in process of forming before my eyes, though to me still unreadable. Having as yet no knowledge of this alphabet, chemistry, I began to draw more remote inferences from grouped observations, and to generalize. Here were two neighboring volcanoes, each showing a different kind of activity; in one the activity was constructive, in the other it was destructive."

Pumpelly thus was led by his own observations to the inference that "heat and gas and water were active agents in building up one volcano and destroying another," but that there was some other factor beyond his reasoning. He traversed the volcanic formations around the bay of Naples until he could have made a geologic map of them, had he known how, and the field of volcanism, with all its diverse phenomena, became correlated in his mind as a whole.

The invaluable character of this effort to unravel a skein of intricate geologic relations, even though with one-eyed understanding, was later fully realized by Pumpelly. It may have been with the kindest purpose that in 1879 he sent a very young and wholly inexperienced geologist, without assistance, to investigate the geology of the iron ores of the eastern United States. It is certainly a testimony to his kindliness toward young men that he never uttered a word of criticism of the results.

But Pumpelly's interests were never single. Referring to his roamings around the historic bay, he says:

"The stately lines of Virgil haunted me and, lying on the citadel mound of old Cumæ and on the heights of Baiæ, I reveled in daydreams—the Trojan wanderers and the Sibyl, the Phlegraem fields and Avernus, and the brilliant life of Baiæ all became real. Those weeks are in my memory one continuous happy dream."

There followed a summer in Corsica, of which more anon, and the autumn of 1856 led the wandering youth and his mother, who had been his constant companion wherever it had been practicable, to Vienna, where he by chance strolled into a meeting of the Deutscher Naturforscher Verein. It was a turning point in his life.

Noeggerath was then a great name among German geologists and it belonged to a kindly old man whom Pumpelly happened to address on entering the meeting. Their conversation resulted in an acquaintance and excursion together and Noeggerath, recognizing the boy's qualities

and enthusiasm, advised him to go to the Royal Mining Academy in Freiberg, Saxony. His advice was accepted and thus the decision was made between the interest in geology and the attraction toward a wider, general education, which might have led to concentration on history and ancient literature. Even so, Pumpelly's inclination toward studies in those branches of science which deal more directly with men, such as ethnology and the evolution of civilization, was never lost to him. It led him during his first journey across Asia to grasp the problem of the ancient migration of the Aryan race, and later, in his seventieth year, to return to those deserts to search for buried cities in the kurgans of Turkestan.

Freiberg, 1856 to 1860

Freiberg in the middle of the nineteenth century was approaching its centennial (it was founded in 1765) and had great reputation, as well as established traditions of training, for the profession of mining. It was the foremost school of practical mining in existence. It drew students from all countries. To be a Freiberg graduate was to hold a diploma of the highest grade in the profession. Having been established at Freiberg because of the ancient mines still being worked there, it was an outgrowth of practical experience, and practice dominated the then relatively simple theories of mining and metallurgy in the curriculum of the school. Today the requirement of scientific knowledge of geology, chemistry, and physics, in their vastly more complex applications to the arts, has wrought a change in the training of the mining engineer and metallurgist. Although some great schools still use working models of machinery and other equipment for demonstration, even these tools have been subordinated or abandoned in the more advanced curricula, and training in practice has given place to founding in principles. Things have changed at Freiberg also. The mines, after having been worked for some years in order to maintain the school, which sprang from them, have been shut down, and with the growth of mining schools in other lands, especially in America, the former center of the art has lost its pre-eminent position. In Pumpelly's student days it had, however, no equal.

The old Freiberg held another tradition worthy of imitation and which Pumpelly greatly appreciated. It was *Gemuetlichkeit.* Militarism had not yet chilled the genial German spirit. Authority and learning, instead of presenting a barrier to youthful aspirations, opened the doors and pointed the way blazed by experience. Cotta, Weisbach, and Breithaupt met their students in the true fellowship of knowledge and became

in a sense their comrades while retaining their respect. Pumpelly entered readily into this comradeship. Referring to the frequent excursions taken with Cotta, he says:

"During the years I was in Freiberg these trips covered not only the neighboring region, but extended to Bohemia and Thuringia, sometimes lasting a week or even longer. In some of these, in the holidays, several of the other professors joined and the whole became a jovial picnic. These men, old and middle-aged, were quite as good comrades as any of us. Outside of the lecture room they were young as we were; we liked to mingle with them at the restaurant tables, drinking beer, smoking, or telling stories, or in serious talk; and they joined us in the same spirit. I hold this to have been an important element in education. It is a phase that seems to be lacking in our universities and is only partially represented in our conferences on special subjects. Professor Shaler, of Harvard, was the only American instructor I can recall who was a representative of the type of these men in that respect. Bless them and him, for they are remembered with affection and gratitude."

It may not have been difficult for Pumpelly's generous spirit to learn this lesson of encouragement for the young. That he did learn it and practiced it in all his relations with associates of every degree, none who has come into contact with him would fail to testify. It was his influence, joined to that of Chamberlin, Gilbert, and Powell, which introduced into American geology the habit of give and take in research. It has become so general that he who does not exchange ideas with his fellow or fails to give credit where credit is due achieves something of the unenviable reputation of a miser, as one who is to be condemned for his avariciousness of thought and to be pitied for what he loses.

It was typical of Pumpelly's impatience of close thinking that he never acquired a thorough knowledge of mathematics. This was a great drawback to him, on entering Freiberg as well as later, but as a student he took heroic measures to overcome his deficiency. Finding himself obliged to take private lessons and having no time during the usual waking hours, he hired a student to bring him a tub of cold water at half past three in the morning, pull the bedclothes off him, and start the fire in the big porcelain stove—all under pain of quick dismissal if he failed. This discipline was kept up during three years and was followed each morning by a lesson in mathematics over the coffee cups. The procedure was, however, only partially successful. Pumpelly never developed a memory for detailed minutia, such as the mathematician must possess. He saw the world as the condor sees it, yet could focus his vision intently for the moment.

Cotta seems to have been the chief influence in his studies at Freiberg, but Breithaupt interested him in crystals and in their systematic rela-

tions. Teaching crystallography without mathematics and by the use of models, he pointed out the relationships, an understanding of which transforms the barren description of figures into a fertile field of thought. The study of paragenesis and pseudomorphs, pursued under that able teacher, was developed later in Pumpelly's contributions to metasomatism, especially of the Lake Superior copper ores.

It is difficult after three-quarters of a century to translate our thought to the stage of development of the sciences in that day. We no longer refer to the writings of the men who were then the ultimate authorities: Humboldt, von Buch, Cotta, Lyell, Murchison, Elie de Beaumont, d'Orbigny. Science was then exceedingly simple, but it opened a vast prospect, a fascinating opportunity to a young man with a genius for exploration.

Pumpelly remained at Freiberg until 1860, when, having completed his courses of study, he returned via Paris to America. He had left home as a boy of seventeen; he came back not only grown to manhood, but with a knowledge of the great world, unspoiled yet experienced. Among his companions at Freiberg had been James D. Hague and the brothers Henry and Louis Janin, all three of whom achieved enviable reputations in their profession. They, however, pursued its more technical opportunities, while Pumpelly, after a first essay at mining, struck out into the broader fields of geological exploration.

Corsica, 1856 to 1857

It was foretold of Pumpelly, when he was but seventeen, that he would make long journeys by sea and through dangerous lands, but that he would escape, unless taken unawares from behind by some one of his companions. He tells us that the saying made him cautious as to conditions in his rear and elsewhere—that is, it put him on the alert and enabled him to pass safely through various dangers. Whatever the effect of the prophecy upon his imagination, he undoubtedly owed much to his keen, highly developed habit of observation. Again and again in the story of his life it happens that an incident, too slight to be commonly noticed, gave him warning and enabled him to act effectively, in time.

Two summers spent in Corsica, one as a rambler on the lookout for bandits, the other as a companion of the bandits on the lookout for gendarmes, introduced him, while still a youth, to the thrilling delights of a life of romantic risks.

He first saw Corsica while en route from Marseille to Naples, and as his steamer slowly passed the peaks, which rise to nearly ten thousand

feet above the Mediterranean, he longed to roam among them. The realization of his desire came about in a manner so characteristic of himself that it can not be omitted.

One beautiful day in May, 1856, Pumpelly, having grown weary of Florence, where he was with his mother, left her for a day or two at most, not knowing whither fancy might lead him. His impulse took him by train to Leghorn; then by a chance steamer to Bastia, in Corsica, and on to an idyllic visit with a forester and his charming Parisian wife; thence he climbed among the mountains, wandered carefree with the Corsican shepherds, shared their primitive life, and steeped himself in the wild beauty of the heights and in freedom. In September he returned to his mother, to find that his occasional notes, sent back through shepherds, had never reached her, and he was mourned as dead.

There are no more fascinating chapters in Pumpelly's reminiscences than those which describe his two summers among the Corsican shepherds, in whom he found the stern character and virility of ancient Rome, the piety of the early Christians, and the fierce spirit of the Italian vendetta. But only the geologic observations which he made incidentally are appropriate here.

During his early excursions he penetrated to the heart of the island and saw for the first time the core of a granitic massif laid bare by the profound gorges. Later he reached the porphyries of Monte Baglia Orba and observed the variations of color and texture which distinguished the, to him, still unknown rocks. He recognized them, however, as the same that he had seen in the ruins of the palace of the Cæsars in Rome. Noting that one dike would cut through another led him to infer differences of age between the two, and he became interested in tracing out these relationships. The sketches he then made in his notebook (he was but nineteen) afterwards helped to introduce him to Noeggerath, and thus to open the way to Freiberg. During his second visit to the islands he made a systematic study of the porphyries and correlated the dikes according to their differences in age and the rocks they traversed.

Pumpelly also recognized evidences of glaciation in the mountains of Corsica, and thus established the fact that the Glacial Period had left its traces there. His account, published in the Neues Jahrbuch für Mineralogie, describes the polished and striated surfaces of the hard porphyries, the transported blocks of rocks not found in place on the slopes where the erratics lie, and a terminal moraine. Referring to three great fragments, 30 to 40 feet through, which were so piled on each other as to form a cave in which he camped with shepherds, he argues:

"Now that one no longer believes in floods of boulders, what transporting agent other than glaciers can have operated here to carry these great rocks down a valley where the average slope is not far from 8 degrees, and to have left them in their present positions without having rounded them off in the least."

We must remember that Agassiz's hypothesis of glaciation was then in its infancy, and what is today a commonplace of elementary science was an unfamiliar, somewhat daring inference; but, accepting the facts he observed, Pumpelly ranged on into the realm of hypothesis, as was ever the habit of his far-reaching imagination. He reasoned that glaciation should depend on heavy precipitation, as it does, and he sought for geographic conditions to which a greater precipitation in Corsica at an earlier epoch might be ascribed. It was generally believed that the Sahara Desert had formerly been covered by a sea, and he found in that hypothetical water body a source of moisture which the southerly winds should have carried to the Pyrenees, the Alps, and Corsica. He sought confirmatory evidence, not without success, and he was encouraged by Cotta and Reich, his instructors, to write out his hypothesis. They even suggested that it was worthy of the degree of Ph. D. from Heidelberg. Nevertheless, when he came to write down his observations he excluded the theory, because he had no definite evidence that there ever had been an inland sea covering the Sahara. Thus an instinctive caution characteristic of the true scientist withheld the young investigator in his first original essay from falling into error.

In these his earliest steps in geology Pumpelly employed two methods which afterwards characterized his geologic studies and which he has passed on. He grasped the salient facts in bird's-eye view, and from them framed an explanation, an hypothesis. This he tested, modified, proved, or discarded, as additional facts appeared, going on, *pari passu,* to another hypothesis if the first was proved wrong. It is the method of *Sequential hypotheses* in contradistinction to that of *Multiple hypotheses.* The former is as essential and as appropriate to reconnaissance work as the latter is to exhaustive research. The capacity to seize essential facts and to detect their correct relations at a glance is vital to success in exploration and is an attribute of the great explorers. But it requires to be held in check by detailed studies of key phenomena, of those details in which the true relations are demonstrable on a small scale, perhaps, or for a single locality. Only thus can the observer avoid superficial and probably erroneous conclusions. The power thus to guard against false inferences by painstaking investigation of details of key facts is too often lacking in men of the eagle eye, but Pumpelly had it. It was demon-

strated even in his earliest work by the care with which he sketched the relations of the porphyry dikes of Corsica.

Arizona, 1860 to 1861

Arizona in 1860-1861 was a land of hell and sudden death. A majority of its white inhabitants cared little what came after. They knew no law save their own caprice; were exiles from that portion of the frontier where laws could be enforced. Mexican peons constituted another part of the population, indispensable laborers, dangerous cut-throats. The small body of United States soldiers, who had kept a semblance of order, were withdrawn in the spring of 1861, leaving the ranchers and miners to that extermination which had been sworn by the Apaches, whom outrages of every sort had driven to desperation.

Into this lawless land came Pumpelly as mining engineer in charge of the Santa Rita Mine. He was twenty-four. Behind him were Freiberg, Vienna, Rome, and Paris; Corsica also; and if in that far island he had lived among outlaws, they were so only in opposition to the established order. Their own code they obeyed implicitly. Here was a land without law, where a man's only safeguards were physical strength, cool courage, and alertness. Pumpelly possessed these qualifications in high degree. He could ride and shoot, but, most important, he let nothing escape his observation. His cat, sniffing the breeze, warned him of Indians who had just killed his associate, Grosvenor. He himself was one of the very few mining engineers who escaped massacre.

The problems presented by the geologic structure of the mine and the smelting of the ores fully occupied the young engineer during his first few months at the mine. The Santa Rita was reputed to have yielded fabulous wealth to the Spaniards. It was expected to yield accordingly to the new American company, which, like many another, thought to acquire riches for nothing. The working capital was very limited, equipment there was none beyond what the natural resources of the region afforded. The richness of the ores had been exaggerated or the richer ores had been exhausted. The general facts of the geology, the existence of two distinct groups of mineral deposits of which the older was the more valuable, the general character of the ores and their proper metallurgical treatment, none of these now well-known things had been worked out. Pumpelly faced an intricate complexity of problems, which he must solve unaided and without essential materials. He has put the situation into a paragraph:

"We needed fuel, fireproof furnace materials, machinery and power, and the supply of these furnished by Nature in Arizona was of a kind to necessitate

a great deal of trouble and experimenting when taken in connection with the peculiar character of our ore. The season was promising to pass without our hacienda being troubled by Indians, when one morning our whole herd of forty or fifty fine horses and mules was missing. Several times during the remainder of the winter and spring we were attacked by Apaches and our mines were the scene of more fighting than any other part of the Territory."

When the Territory was abandoned to the Indians by the withdrawal of the soldiers, those in charge of the few isolated mines which had been opened could only take such measures as might most speedily and safely enable them to save the movable property and escape. Money was needed and could be had only from the ores. The Santa Rita had no sufficient supply, since the Indians had for some time made it impossible to work the mine, but payment of a debt due from the Heintselman Mine was accepted in ore worth $2,000 a ton. It cost the lives of two Mexicans and of Grosvenor to get the ore to the Santa Rita, where Pumpelly and Robinson, the bookkeeper, were thus left to carry on with the aid of a chance American and their unpaid Mexican peons. The Mexicans, armed with rifles to withstand the Indians, were scarcely less dangerous and were not allowed to cross a dead line. Wood had to be cut, hauled in, and burned to charcoal. The furnaces, standing on a point between two ravines, lighted up every object near them and exposed the workers constantly to the fire of the Apaches. Pumpelly's chief smelter was shot and he was obliged to finish the separation of the silver and lead almost without sleep for fifty hours or more.

"The two other Americans, revolver in hand, kept an unceasing guard over the Mexicans; whose manner showed plainly their thoughts. Before the silver was cool we loaded it. We had the remaining property of the company, even to the wooden machine for working the blast, in the wagons and were on the way to Tubac, which we reached the same day. Here, while the last wagon was being unloaded, a rifle was accidentally discharged and the ball, passing through my hair above the ear, deafened me for the whole afternoon. Thus ended my experience of eight months mining in an Apache stronghold."

Truly he may be said to have borne a charmed life.

Geologic studies could not well be carried on satisfactorily under the conditions which governed Pumpelly's movements during that adventurous eight months. Yet he assembled observations bearing on the rocks with which the ores are associated, as well as on the mineralogical nature of the ores themselves and on the sequence of the minerals. These he laid before the California Academy of Sciences in August, 1861. In view of the great advances which have since been made in all branches of mining geology and of the development of the mining districts of

Arizona, his essay is chiefly of historical interest, a milestone from which to measure how far we have come and what a fruitful epoch in science his long life covered.

Japan and China, 1861 to 1864

At the age of twenty-four Pumpelly was a man of fine physique, combined with a frank, easy manner and a record of achievement. He had been a leader among the students at Freiberg, not because he sought leadership, but through his native qualities—initiative, courage, ability, and good fellowship. His wide and varied associations with all classes of society had given him the bearing of a gentleman of the world, a gentleman without blemish. His unique experiences in Corsica, described with the gift of a born raconteur, had early given him a reputation for daring and geologic understanding, and his success in salvaging the property of the Santa Rita Company under desperate circumstances had confirmed that reputation in America. He was already a prominent figure in his profession.

Thus it happened that he was one of two experts appointed by our Government in response to a request from that of Japan for a geologist and a mining engineer to explore certain lands in southern Yesso and, possibly, to introduce foreign methods of mining and smelting.

Feudal Japan pondered long and gravely the vital question of rank before receiving the experts, Pumpelly and W. P. Blake, but upon assurances from our Minister finally accorded them distinguished standing, such that they were called upon first by the Governor of Yesso. It would have been practically impossible for them to have executed their commission otherwise, since they could hardly have escaped insuperable opposition, if not death, from the natives, had they not been guarded accordingly. There was with very good reason a strong anti-foreign party in Japan, which opposed every move of the weak central government toward opening the empire to the outside world, and the incident of the engagement of the two experts was used as a ground of attack by the great nobles. They accused the Taikoon of throwing the resources of the country open to foreigners, and after a year forced the government to bring the engagement to an end.

Pumpelly has left the record of his observations in Japan in his book of travel, "Across America and Asia," and in the work "Geological Researches in China, Mongolia, and Japan." The former is a graphic description of the strange peoples, their lands, their cities, customs, and politics, sympathetic in feeling and liberal in attitude. The latter consists, so far as Japan is concerned, of geologic itineraries in the island

of Yesso. The author describes them as hasty jottings, made during reconnaissance journeys, at a time when he expected to make a much more thorough study of the geology of Japan. They served, nevertheless, to enable him to distinguish the foundation rocks of the island, a sequence of metamorphosed sediments penetrated by granitic and basic eruptives, and the superjacent volcanic deposits of various kinds. His studies in Corsica and of Vesuvius thus came to his aid on the other side of the globe. He also noted the coastal terraces and identified them as recently elevated, finding marine shells in which the organic matter was not entirely decomposed. The distribution of the various rocks, so far as he was able to observe them, is shown in a sketch map, one of the earliest geological maps of Japan.

Driven out of Japan by the daimios, whose opposition to the more liberal policy of the Tycoon led practically to a state of civil war, Pumpelly proceeded on his own resources to China, to gratify his taste for exploration in that unknown land, so recently opened to modern travelers.

In speaking of the reopening of China, we are apt to forget that between 1541 and 1720 Jesuit missionaries penetrated to every part of the Empire, that they exercised an important influence at the court, and almost succeeded in converting the Emperor himself to Christianity. China was then open to Europeans, but was closed by the anti-Christian agitation about 1720 and remained so for more than a century. During that time China stood still, while Europe advanced by leaps and bounds. America stood in the vanguard of that advance, and Pumpelly, coming from the frontiers of America, yet equipped with the science of modern Europe, looked upon the ancient people through youthful eyes, the eyes of hope and fair play. "Across America and Asia" contains a chapter on the Chinese as emigrants and colonizers, in which the author analyzes the relations of the Chinese and Caucasian races with an intimate understanding of the social and economic problems, and the dedication of that work, written in 1870, expresses the optimism with which he characteristically regarded the long future:

> "As so many of the following pages relate to experiences illustrating the wisdom of that diplomatic policy which, in bringing China into the circle of interdependent nations, promises good to the whole world, I dedicate them to the chief author of that policy—to Anson Burlingame."

Fifty years have passed since that was written, and the impartial observer must admit that much evil has developed from the meeting of the races, as well as good. Nevertheless, one who has trodden in Pumpelly's footsteps, who knows how often the patient, long-suffering Chinese have

risen Phœnix-like from chaos, and who believes in the inevitable supremacy of justice and fair play among Americans, must agree with Pumpelly in his faith in the ultimate preponderance of good to flow from the establishment of mutual relations among the races of men.

Pumpelly was the pioneer geologist of China. The scientific labors of the Jesuits were purely geographical and could not, in view of the lack of geological knowledge at that time, have yielded any understanding of the rocky structure of the country. They did serve, however, to furnish that indipensable prerequisite to geological research, a base map on which to present the geographic relations of the facts. From the Jesuits down to Pumpelly's day certain diplomatic missions had been permitted to traverse the interior, and it had not failed that botanists and zoologists had in a few instances penetrated the land; but such journeys were undertaken only by special permission or at very great personal risk.

From 1860 to 1862 there had been in China a German diplomatic mission, attached to which was a young geologist, Ferdinand von Richthofen, who was eager to explore. He found the conditions so unfavorable, however, that he confined his movements to the coast. Completely thwarted in his efforts, he visited the East Indies and finally crossed the Pacific to California, where he remained until 1868. He had then developed a plan for a systematic exploration of the Chinese Empire and, backed by Professor Whitney, he secured funds for an expedition from the Bank of California. Returning to China, he entered upon his task, to which during four years he devoted all his energies, with the result that his name, Von Richthofen, is identified in the annals of science with the unveiling of the "Middle Kingdom." His researches into the physical geography and geology of the country continued throughout his life, and he left a third volume of his great work, "China," unpublished at the time of his death, in 1905.

In 1863, after Richthofen had left for California, Pumpelly arrived in Shanghai, where he remained some weeks. Seeing on the Yangtze river a boatload of excellent coal from far in the interior, he was seized by a longing to explore. In his "Reminiscences" he says:

> "Excepting missionaries, few travelers had penetrated far into the interior. Huc had descended the Yangtze from Tibet. Blakeston had described that river. The geology of the Empire was absolutely unknown, for Richthofen had not yet undertaken his monumental work. So here, as in Corsica, yielding to the call of the unknown, I engaged passage on the steamer *Surprise*, bound for Hankow, the end of steam navigation up the Yangtze."

Thus, like a boy starting on a pleasure trip, Pumpelly struck out into a country devastated by nine years of rebellion and scarcely less lawless,

so far as foreigners were concerned, than the Arizona from which he had so recently escaped. He was not in search of adventure. Danger was merely incidental to satisfying that interest he felt in the strange people and their unknown land. But neither did danger deter him, either out of regard for his safety or because it would interfere with the attainment of that knowledge which he sought. In this latter respect he differed from Richthofen.

It is interesting to contrast these two great explorers, who had so much in common, yet pursued their ends so differently. It was the writer's privilege to know them both. They were equally bold in conceiving their plans, equally broad in their interests in the Chinese as a people and in China as a land. They differed radically, however, in their ultimate purposes. Pumpelly sought knowledge for knowledge's sake. He wished to know the as yet unknown, but he felt no impelling desire to inform others, nor any special obligation to do so. In his generosity he gave as freely of his thoughts as of things material and always with the desire that others should derive the greatest possible benefit from the intellectual largesse that he scattered so liberally. It might contain the seed of a vital contribution to science; he who could cultivate it was welcome to the reward of its harvest. With Richthofen it was otherwise. He was a guardian of learning. He felt responsible for it. In its pursuit he was farsighted, systematic, patient, and persistent. What he acquired he cultivated. His harvest he shared with those whom he deemed worthy. To do so and to welcome any contribution of fact or of right inference, these were part of the obligation laid on him by his station and his standing.

The difference between the two was temperamental and not merely individual, but racial. Pumpelly derived from the roving Vikings, happiest when driving care-free down the winds of opportunity beyond the ken of men. Richthofen was a modern Roman, obedient to the system in which he had been drilled and of which he knew himself to be a leading exponent. They typify the spirits of America and Prussia.

Pumpelly's excursions in China were limited as compared with Richthofen's journeys. He did not wait for favorable conditions, but even he, when low with fever, could be turned back by a Chinese mob. Thus it happened that he and his companion, the Rev. Josiah Cox, cut their houseboat adrift before the charge of thirty thousand and abandoned the examination of the coal fields of Hunan, the province in which the natives were most violently opposed to foreigners. They were not deterred, however, from proceeding on their voyage up the Yangtze, which they ascended to the head of the Lower Gorges. Pumpelly was deeply

impressed by the superb scenery, where the huge river coils through the narrow pass between towering cliffs of limestone. He watched with intense interest the progress made by a hundred and fifty coolies, who dragged the junk up against the tumultuous current, as had been the custom for thousands of years. He probably felt a thrill not unmixed with delight when the bamboo ropes parted and they went swirling down-stream, for who could know what would happen next?

Eight days were required to warp up through the Gorges, and Pumpelly observed the vast arch in which the strata, that at Ichang emerge from under the plain of the Yangtze, rise and descend again, after arching to such a height that the underlying granite is exposed in the center. The axis of this arch, or anticline, trends northeast-southwest, as Pumpelly noted. Subsequently he connected it with the marginal ranges of the Mongolian plateau and other features having the same trend and thus identified a major structural feature of the Asiatic continent. He gave it the name of "Sinian" trend or axis, from the ancient Hebrew name *Sinim* for China. He thus recognized and named one of the cornerstones of the geology of Asia, both the structure and the name having been accepted by all later observers.

The limestone formation which constitutes the walls of the gorges impressed Pumpelly with its extraordinary thickness. The grandeur of the cliffs suggested the vastness of the seas in which the strata had been deposited and made an impression that influenced his views on the area covered by it. He estimated the thickness of it at 11,600 feet. It has since been determined to be about 8,800 feet, including a parting of shale. He found no fossils, but thought that certain Devonian forms which had been identified from China must represent it, and therefore assigned the entire thickness to that period. We now know that the lower part belongs to the Cambrian and Ordovician ages, while the upper limestone pertains to the Carboniferous.

The recognition of the Sinian trends and the assumption of a wide-spread Devonian limestone gave a clue to the general structure of China, which Pumpelly embodied in the first geological map of the empire ever attempted. Beneath the "Devonian" limestone he grouped all the rocks in a "granito-metamorphic series." The Coal Measures he placed in the Triassic on the evidence of plants identified for him by Newberry. The alluvium of the eastern plains he assigned to the "Tertiary and post-Tertiary." He traced the distribution of these few formations by the occurrence of characteristic minerals, as reported to him by Chinese scholars whom he employed to search the records for that purpose. Thus he expanded his own meager observations to a sketch which, if necessarily

erroneous in many ways, nevertheless contains a large proportion of correct inference.

In turning to Chinese records for information regarding the distribution of mineral resources, Pumpelly was not deterred by the volume of the material, which was enormous, nor by his ignorance of the language. He found natives to glean the scanty data from the huge volumes of ancient and recent writings and missionaries to translate them. It was a characteristic effort, due rather to his German training than to any personal inclination toward exhaustive research, but one which was frequently repeated in the course of his career. He appreciated the value of details, even though he himself would only reluctantly go through the drudgery of digging them up. He willingly left to others both the labor and the credit. Van Hise, a master of detail, whose volumes outbulk Pumpelly's writings a thousand to one, presents an interesting contrast. He also employed others to work with him, but very differently. Meticulously accurate and systematic, he planned his work far in advance, estimated his own ability to execute the more essential part, and availed himself of the aid necessary to secure completion within the allotted time. Single in purpose, he completed each task according to a preconceived plan. Pumpelly, on the other hand, conceived a fruitful thought as lightly as a boy sees a vision in the sunrise, as lightly he gave it wing, and he was happy if another thought it worthy of attention. He congratulated himself that there were others trained and more than willing to follow out the lines of investigation which he spontaneously suggested, leaving him free to turn to any one of the manifold interests which claimed him.

Pumpelly's journeys in China during the autumn of 1863 were accomplished under official escort in search of coal for naval use. They served to broaden his understanding of the relations of the Coal Measures with the great limestone formation and gave him the foundation upon which he expanded his observations in the Yangtze Valley to northern China. They also afforded him material for amusing chapters in *Across America and Asia.* He spent 1864 partly in China, partly in Japan, and in November of that year started to return to America by crossing Mongolia and Siberia to Europe. On this journey he made those observations of the inland basins of Asia and of the disintegration of rocks in an arid climate which appeared subsequently in his theory of the loess as a lacustrine deposit.

In his scientific contributions to the geology of Asia, Pumpelly apologizes for the often meager results on the ground that the expeditions were carried out with private funds and were attended with difficulties

due to the hostility of the natives. He also mentions the intense cold of a winter journey across the plateaus of Tibet as a limitation to geologic observation. To these we may add the very inadequate state of geologic knowledge and theory when considered as a key to the problems he had to face. Thus he reasoned regarding the origin of the rock-rimmed basins of central Asia. Having no outlet, they could not be stream-eroded valleys. Wind, however, was an obvious eroding agent which could excavate basins, and so he attributed to the wind effects whose magnitude surpasses its reasonable activities. There was then no hint of the physiographic clue by means of which we have since learned that the surfaces of the continents, and notably of Asia, have been warped recently in a way which gives rise to wide depressions as well as to broad plateaus.

He recognized that the Yellow River in its north-south course, before it turns east toward the plains, follows an unusual channel across east-west mountains, but since science knew nothing of the changes which may be imposed on a river by stream-capture, he sought an explanation in a catastrophe, an upheaval, of which he found suggestions in very ancient Chinese traditions.

He was impressed by the enormously wide distribution and thickness of the fine-grained loess deposits and, following the accepted theory of lacustrine origin, he postulated the former existence of extensive lakes. Thus he reasoned according to the state of theory at that time, and failed to find the correct solution of the problems because they were beyond the reach of contemporary understanding; but he grasped the problems.

Lake Superior, 1865 to 1877

Upon his return to America in 1865, Pumpelly encountered the changes wrought by the Civil War. With the exception of what may be called a brief visit to his home in 1860, he had been away sixteen years—years of growth from boyhood to maturity for any man and for him years of extraordinary experience. Writing of it fifty years later, he refers to the desultory character of his schooling, comprised in the preparation for Yale and three years at Freiberg, and stresses the value of that broader education which flows from intercourse with the world:

"One can not in young manhood be in more or less intimate and confidential and unprejudiced association with men and women of many peoples without getting some insight into the complexity of human nature, into the relation of virtues and weaknesses to subconscious motive forces. Such experience makes for introspection and charity.

"I had learned also the sameness of the fundamentals of human nature,

whether evident on the surface in the savage, or veiled by conventional restraint in civilization, or again in the white man when unrestrained beyond civilized environment. I had employed men of many kinds and races—Corsican mountaineers, Indians, Mexicans, white men (both normal and outlaw), Japanese, Chinese, and Mongolians—and had learned to understand racial character well enough to get on smoothly. In Japan, in official position, under the feudal régime, I had established intimate, in some cases affectionate, relations with my staff of Samurai officers. In extended travels in China my experience had ranged from the receipt of courteous hospitality to the necessity of turning a hostile mob into a friendly one.

"In commercial Shanghai I had seen the causes of dislike of the foreigner. In Peking I had purposely been allowed to see at first hand the attempts of a broad diplomacy intended to remove those causes. And as the head of an Imperial Chinese commission consisting of civil and military mandarins, I had come in contact with local magistrates and had seen something of Chinese character in dealings of the central government with the local democracy.

"I had seen nature in all its aspects, of mountain and desert and cultivation, between the tropical and Arctic circles, and its corresponding influences on man and civilization.

"These were some of the factors in my deeper education up to my twenty-eighth year. If I had been conscious of the fact that I was going through a great school, I should undoubtedly have profited vastly more than I did. However, in its gradual growth that school had helped me through dangerous or delicate situations, and had moved me to give weight to the better instincts of men of all races when those instincts are properly appealed to. It had developed in me a racial sympathy and made clear the dangers of racial prejudice."

Thus, before reaching the age of thirty, but a third of his life gone by, Pumpelly had graduated from the school of human nature with the degree of *advocate of tolerance.* He is a rare exception to the rule that most men never enter that school understandingly and few get beyond its most elementary lessons, however long they may live. Those who attain to his degree are apostles of the millennium.

The Civil War left a number of men in the prime of life accustomed to danger, initiative, and large undertakings. A post-war period is intensely stimulating when the exhaustion of resources has not gone too far, and in the United States the vast wealth native to the country was scarcely touched. It was an environment in which Pumpelly found many congenial friends and opportunities for the exercise of his special gifts and training. He did not immediately enter upon the practice of his profession, but gave himself up to the enjoyment of society, where his personal charm, his geniality, and his broad knowledge made him universally welcome, and to the writing of his two books, "Across

America and Asia" and "Geological Researches in China, Mongolia, and Japan."

He became closely associated with Whitney, whom he had known in California, and through him he was invited to accept the Chair of Mining at Harvard under the Sturgis-Hooper Endowment. As the available income would not suffice to pay him a professor's salary, Pumpelly accepted; his duties would be light and he would have much freedom. He is therefore listed as having been a professor at Harvard, but he never entered seriously upon the work of teaching in the university. Inspiring as he was as an instructor in the presence of Nature, he was ill fitted for systematic teaching in an institution.

In the summer of 1866 Pumpelly made a first excursion to Lake Superior to report on a copper property, and became acquainted with the possibilities offered by exploration of the iron ore ranges as well. He was by nature a prospector, and here was a prospector's chance, upon which he prepared to embark, risking his own and his brother's resources. But, given an opportunity to locate a floating land grant, he was enabled to do justice to his employers while at the same time he studied the conditions of occurrence of the iron ores and became acquainted with lands they could not acquire. Thus left free to exercise the right of second choice, he threw himself into the exploration with the definite purpose of investing his savings in timber and iron lands. At this time he became acquainted with Major T. B. Brooks, who had already worked out the geology of the Marquette iron district, and the two entered into an informal association, which lasted without any written contract for nearly forty years. Confiding each in the other, they invested from time to time such funds as each could individually spare in lands that one or the other of them might select.

Brooks was a self-made engineer and geologist, who by force of character, ability, and indomitable energy had developed himself from a farmer's boy to the position of vice-president and general manager of the Clifton iron mine on the practical side and in the scientific world to that of the leading authority on the geology of the iron deposits. He was a year older than Pumpelly—that is, just thirty—when they met; but, having spent three years under the stern discipline of war and possessing a far more exacting habit of life, he seemed much older by comparison. The two men came together on the common ground of their interest in exploration with both practical and scientific objects in view. They became united by bonds of mutual respect and confidence, which were never broken. Their families came into relations of intimacy. Yet there must ever have remained in each a phase with which the other

could not readily sympathize. Brooks was intensely practical, definite, and devoted to the business in hand. Pumpelly was habitually impatient of details, eager to be done with them, so that he might be free to roam in any one of the intellectual fields so attractive to his brilliant mind. Ten years later the writer received his earliest training in field-work from these two geologists. Brooks might have been a father, Pumpelly Phœbus Apollo.

The State of Michigan was fortunate at this time, when the exploitation of its resources was just beginning, to have two such men as Brooks and Pumpelly interested in that development. They accepted the tasks of surveying the iron and copper districts and laid sound geological foundations for prospecting and mining. The insignificant appropriation made by the State was inadequate even for expenses. The investigations were thus a gift to the Commonwealth as well as to science.

Pumpelly undertook the study of the copper deposits. The limitations of the work required that he should decide between a general reconnaissance of the forested and drift-covered area or limited but accurate observations of certain sections and deposits. In contrast to what one might have expected, he chose the latter, rightly judging that specific information regarding the stratigraphy and conditions of mineralization of the copper-bearing rocks would be of more value than loose generalizations, such as had characterized the exploratory work of his predecessors. The problem was one for which his German training had prepared him advantageously.

Delegating the measurement of sections across the series of strata to his assistant, Marvine, he himself undertook the investigation of the broader stratigraphic relations, especially those of the bedded copper-bearing sandstones and volcanics with the Cambrian, which had been described as conformable. This required field observations of a kind in which we might expect him to excel, as he did—observations demanding the correlation of facts assembled from a wide area and interpreted with keen perception of their relative value and significance. He recognized the now well-known unconformity between the Keweenawan and Cambrian sandstones and published jointly with Brooks an article on the age of the copper-bearing rocks.

Pumpelly's major contribution to the study of the copper deposits reminds one of Breithaupt, his instructor in crystallography. Seeking the conditions under which the native copper had been deposited in association with quartz, epidote, prehnite, calcite, and other minerals, he examined under the microscope hundreds of thin-sections, which he ground himself, measured the angles of microscopic crystals, and distinguished

the older from the younger minerals by the interpenetration of one by another or by the forms assumed by pseudomorphs. He thus determined the paragenesis of the copper and traced its reduction to the metallic state through the chemical reactions with ferrous minerals present in the associated rocks. He attributed the concentration of the metal to descending surface waters. In this he followed Whitney and has been followed by Wadsworth.* Van Hise was of the opinion that ascending waters had brought in the copper, but this is not sustained by the evidence, according to Lindgren. In identifying the copper as indigenous to the series of rocks in which it occurs, Pumpelly thought it to have been derived originally from seawater. Van Hise later called attention to the widespread occurrence of it in the Keweenawan traps and looked on the lavas as the source. Lane has suggested that the submarine outflow of the lavas gave rise to reactions with the seawater, which led to the reduction of the copper. This is a modification of Pumpelly's idea, which postulates ferrous oxide as the reducing agent. Exhaustive research has thus to some extent supplemented or modified the views of the pioneer in this field, but the body of fact and inference which he assembled remains the accepted truth. The surprising thing to one who has followed his adventurous career through Arizona and Asia is that he could patiently devote himself to tedious microscopic studies with such success. He began his work on the copper deposits in 1870 and his last paper on the subject, "Metasomatic development of the copper-bearing rocks of Lake Superior," was finished in 1877.

Pumpelly's work on the Lake Superior copper deposits would present no novelty if carried out today, when the processes of metamorphism have become familiar to all students of ore deposits, but they constituted an original contribution of very great value fifty years ago. Among those who then came under his influence was Waldemar Lindgren, at the time just entering on that career in economic geology in which he has rendered such valuable service to science and mining. He, like Pumpelly, was a graduate of Freiberg and, of Swedish birth, had recently come from Europe when he entered the Transcontinental Survey. He writes:[3]

"I first met Pumpelly in November, 1883, and remained in his office at Newport until February, working during that time on thin-sections of rocks obtained during the field-work of the Transcontinental Survey in the summer of 1883. I was tremendously impressed, both by his knowledge and personality, and I shall always remember his kindness to me, at that time a recent graduate from the schools. His advice on petrographic work was extremely

[3] Personal communication.

helpful, but I was not then acquainted with his epoch-making work on the minerals of the Lake Superior district. As I began to dip into metasomatism later on, I soon found that he was really a pioneer in the study of changes which minerals undergo. Of course, this matter had been discussed previously by Naumann, Blum, and others in Germany and elsewhere in Europe, but Pumpelly was the first to apply the microscope to these problems. He was probably the first one in this country who undertook petrographic examinations by using thin-sections. Neither the microscopes nor the sections were as good as they are now, but it remains as a great credit to his genius that the conclusions he arrived at stand, with minor exceptions, practically unchanged today. His work showed great originality, and in this direction of the microscopic study of minerals he was as much of a pioneer as he proved to be in geological explorations. Our geology of today certainly owes a great deal to his pioneering work."

Thus Pumpelly opened up a new line of research in this country, one which has proved a mine of wealth in a material as well as scientific sense. His ideas are reflected in Brooks's early suggestion of the secondary origin of the Lake Superior iron ores, fully developed and established later by Van Hise, and they have borne fruit in the comprehensive studies of secondary enrichment which characterize the investigations of ore deposits during the last two or three decades.

Missouri, 1871 and 1872

Appointed State Geologist of Missouri in the autumn of 1871, Pumpelly took up the task with the intention of remaining in the official position only as long as might be necessary to study the geology and distribution of the iron ores of the State and to prepare the way for a systematic study of the other mineral resources. Being taken seriously ill after a year in office, he resigned in the winter of 1872-1873. His contribution to geology during this eighteen months is comprised in the report of the Geological Survey of Missouri for 1872. He himself wrote only on the geology of Pilot Knob and vicinity and he confined himself to a description of the occurrences and kinds of the iron and manganese ores. It was a preliminary, perhaps, to a systematic study of their origin, but if so it was never carried further. In resigning, Pumpelly left a good working organization, which was taken over and carried on by his successor, G. C. Broadhead.

Loess and secular Disintegration, 1863 and 1876

Earlier in this memoir reference has been made to Pumpelly's observations of the widespread deposits of loess in China and central Asia, which he explained as of aqueous sedimentary origin, consisting of glacial mud

laid down in a chain of lakes. It was not known in 1863 that northern Asia had not been extensively glaciated, and the topography of the continent had not been mapped with sufficient detail to render improbable the former existence of great lakes. Richthofen's extended journeys gave him a better opportunity to observe the extraordinary facts of the distribution of Chinese loess and led him to the correct hypothesis of its carriage and deposition by wind.

The origin of the material of which loess consists Richthofen sought in the disintegration of rocks under changes of temperature and in wind erosion of soft desert formations rather than in glacial erosion, as Pumpelly had done.

But both of them in their earlier thought had failed to recognize that the processes of grinding or disintegration are far too slow to keep pace with the transportation of loess. The current local supply would be exhausted and loess accumulation would cease long before quantities such as exist could possibly be assembled. Pumpelly, reviewing Richthofen's work in 1876, recognized this weakness in the former's hypothesis of loess origin. While he accepted the revision of his own ideas and indorsed as correct Richthofen's theory of transportation and deposition by wind, he brought his observation of the deep decay of rocks in moist climates to bear on the problem of original supply and supplemented the theory at a vital point. He also extended his idea to the origin of ice-transported material. We may best quote his own words from his "Reminiscences" (page 612). He says:

"My argument was:

"(1) That during long ages of normally moist climate the rocks of a region become disintegrated and decomposed often to great depths. This is evident in all regions that have not been denuded by glacial action.

"(2) That any climatic change that permanently destroys the vegetation leaves the disintegrated mass a prey to forces of transportation.

"(3) Through a climatic change causing a glacial period there would come the removal of the altered rock-mass in all the forms of glacial débris—boulders, gravel, clay, and glacial flour, which is the product of the grinding of débris and rock bed under the moving ice. This material is in part left by the retreating glacier as moraine and glacial drift or till, and in part is carried as coarser or finer debris by streams to be spread over the lowlands or carried to the sea. The downward disintegration acting on rocks of different power of resistance would leave an abnormally irregular topography of the solid rock surface in which depressions scoured out by the loaded ice would remain as rock basins.

"(4) If the change is to aridity, the region becomes a desert, vegetation disappears, and the disintegrated rock-mass becomes prey to the winds.

"In Asia the products of glacier work were spread out to dry on the flood-plains of rivers emerging onto the deserts. Thus water had played an

intermediate part in the origin of a portion of the loess. But the final stage of its transportation was, as shown by Richthofen, by the winds that sifted it out from the sands and carried it beyond the barren desert to find rest in the grass, which protected it and which it nourished. Richthofen accepted my view and published it with credit in his second volume."

Here was a broad deduction, based on extended observation and carried to its logical consequences. It embraced the concepts of changing climates, altered conditions of erosion by ice, wind, or water, varied results in sedimentation, and the effect of modified environment on organisms. In Pumpelly's presentation before the National Academy of Sciences he touched upon each of these aspects of the subject and demonstrated his grasp of them. Yet he did not follow them further, did not develop the lines of investigation which radiate from this central group of ideas.

Another great mind, a contemporary of his, similarly brought face to face with the problem of climatic change, pursued its ramifications till led to a new theory of cosmogony. His was the mind of a profound and philosophic thinker. Pumpelly's was that of the explorer in the realm of thought as in that of observation. He glimpsed a truth; he described what he saw from the mountain top; he did not descend to launch his thought on an eddying current; he had no impulse to survey in detail the scenes among which he would not permanently abide.

Yet his argument rounded out the suggestions of other geologists and introduced the climatic factor as an essential element of reasoning regarding the processes of evolution of physiographic features, continental or marine sediments, and the development of organisms, including man and his civilization. In this last-named relation Pumpelly long afterward showed the importance of the climatic factor in his investigations of the ancient cities of central Asia.

Official Surveys, 1879 to 1890

Pumpelly was now forty years of age. He had behind him the experience and observations of an ordinarily full life. Although at times hard pressed for immediate moneys, for he was a generous and care-free spender, he had from his student days up been accustomed to alternating fulness and emptiness of purse, and its condition never long affected his habit of living. He was happily married; he had hosts of friends with whom he shared a variety of interests. Science, literature, history, art, development of national resources, farming, social opportunities—all drew out his abundant interest, gave outlet to his exuberant vitality.

Under these circumstances he was naturally invited to take part in the

larger enterprises in which geology was a factor, not as an investigator, but as an organizer.

The consolidation of the Government surveys of the Far West under the leadership of Clarence King and the plans which the Survey developed in connection with the taking of the Tenth Census gave Pumpelly an opportunity. He accepted the responsibility for the census of the mineral industries, exclusive of the precious metals and petroleum, and directed the forces at his disposal toward a complete investigation of the geology and chemical nature of the iron ores of the country, at least in the more settled regions. This purpose was a natural outcome of his world-wide knowledge of the bases of civilization and his intimate studies of the iron deposits of Lake Superior and Missouri. He fully realized that the future of American industries was involved in the production of iron and steel in competition with the world. He appreciated the need of more thorough knowledge of the distribution of good ores. His mind daringly hurdled the difficulties of so great a task and he confidently organized his division, with very inadequate resources in men and money, it must be confessed.

Four young geologists were sent into the field equipped with the title of "special expert," the humor of which the writer, at least, who was of the number, was too inexperienced to appreciate. A group of chemists was gathered at headquarters, in Newport, Rhode Island. During two years, 1879 to 1881, the field men diligently sought out the iron ore occurrences of the States east of the Great Plains, many of them known only through the tradition of a colonial charcoal furnace. Each occurrence was described as found and any available ore was sampled as carefully as the circumstances permitted. In the case of working mines or stock piles, the method of quartering down large samples composed of many bits of ore taken at random was employed; in the case of an abandoned ore bank, such material as was on the surface was taken. There was no excavating. The observation of geologic associations had to be superficial. Nevertheless, the amount of information gathered, comprising the description of localities and the chemical analyses of more than a thousand ore deposits, constitutes an important contribution to our knowledge of the national resources in this important metal. Pumpelly's share in it lies in the initiative, in the working out of a practical plan within the limited appropriation, and particularly in the inspiration he gave to his assistants. In his "Reminiscences" he refers appreciatively to the *esprit de corps* which ruled throughout the organization. The thrill of it persists after more than forty years. Its source was in his

own abounding enthusiasm and generous confidence in the loyalty and capacity of the young men whom he drew about him.

In 1881, before the Census work was completed, Pumpelly was offered an opportunity than which none could have been better suited to his tastes and abilities. Mr. Henry Villard, president of the newly reorganized Northern Pacific Railway, with rare foresight, realized that the exploitation of the vast empire across which the road was being built would proceed far more advantageously if guided by knowledge of the resources of the region. He appreciated, furthermore, the need of basing that knowledge on facts rather than on hearsay, and he understood that facts can best be assembled by trained observers. He proposed, therefore, the organization of an economic survey, which was to cover the freight resources tributary to his road and which should rest upon studies of the geology, soils, and timber lands. He invited Pumpelly to become its director.

With what enthusiasm that invitation was accepted can be understood only by one who knew Pumpelly's interest in great, beneficent enterprises of a practical character. He was a man of very broad, far-seeing vision. So was Henry Villard, though from another angle. Their two minds worked sympathetically. Here was a vast realm of great potentialities; here were almost limitless plains and mountains, under varied climates, to be opened to human settlement; here were resources of all kinds to be exploited in the building up of new communities; the railway, stretching out under Villard's direction, was to be the instrument and Pumpelly's survey the guiding intelligence of this illimitable enterprise.

It was born too big to live long. The Northern Transcontinental Survey, as it was called, demanded expenditures commensurate with its scope. Mr. Villard supported it most liberally while his power lasted; but when, after three years of intense activity, he failed, just as the rails were linked from terminus to terminus, the Survey fell with him.

We are not here concerned with the causes of the failure, which are to be sought in the rivalries of kings of finance for the control of empires; but it is but doing justice to two able, practical men to record the fact that the Transcontinental Survey far more than paid for itself. It may fairly be estimated that it saved its cost in checking the extortion which was being practiced by officials of the road upon the company (through the reselling of lands privately purchased by them or their friends) and in coal lands that were discovered by the geologists of the Survey and acquired by the Land Department of the railway it has paid for itself many times over, with compound interest on the investment.

Pumpelly threw himself into the work with optimistic energy. In his "Reminiscences" he states that he divided his personal supervision between administrative work and reconnaissance expeditions, to get the broad conceptions of the problems necessary to blocking out plans. His capacity in administration was shown chiefly in selecting his assistants, nearly all of them young men, to whom he gave generously of confidence and freedom of action—an excellent method when the initial choice has been wisely made. His personal direction of the work was limited to brief instructions, commonly given orally, and to occasional visits of a day or two, devoted to rapid reconnaissances of the fields under survey. He trusted us (I speak for my comrades, most of whom have preceded their great chief on the long trail) and he received the response which youth ever gives to faith associated with enthusiasm.

The winters of 1882, 1883, and 1884 were spent by the entire technical force at headquarters, at Newport, Rhode Island, where the younger members enjoyed frequent contact with the director and learned much from his world-wide experience. Pumpelly spent the summers of 1882 and 1883 in Montana and the Rocky Mountains of Idaho. He made two attempts to cross the latter, from the Plains to the Flathead Valley, by way of Two Medicine Pass, from which he had to turn back on account of deep snow, and more successfully by the Cutbank Pass. The latter trip led him across what is now the southern part of Glacier National Park and he first saw the glaciers at the head of Thompson Creek. It does not appear that he made any notable geological observations on any of these excursions. His contributions to science were made through his assistants, who were encouraged to prepare their results for publication over their own names, even after the failure of the Northern Pacific had cut off all support from that source. How the staff was kept at work and paid during the winter of 1884 has never been divulged, but it is a fair assumption that Pumpelly backed his determination to get out the results without regard to the cost to himself. The geology of the coal fields, which had been studied in Montana and Washington, was published in the volume issued by the Tenth Census, Number XV, containing the results of the Census studies, also made under Pumpelly's direction.

Green Mountain Studies

Detailed investigations, requiring painstaking application in field or laboratory, were foreign to Pumpelly's free, roving nature, yet his German training had taught him the value of intensive studies and he showed himself capable of executing them, as in the examination of the

Lake Superior copper deposits. Accident rather than design threw some of the most difficult of American geologic problems in his path. In association with Irving and Van Hise, he studied the pre-Cambrian, and later, as geologist in charge of the New England Division of the United States Geological Survey, he directed the surveys of the Green Mountains of New England, more particularly with reference to Hoosac and Greylock Mountains.

His contributions to the solution of the pre-Cambrian problems, involving the metamorphism, structure, and original character of the ancient crystalline rocks, were made in conversation in the field face to face with the facts or in talks of an evening between days' work. They were not recorded, but his colleagues, notably Van Hise, have borne witness to the value of his penetrating insight into the intricate relations and processes.

Pumpelly greatly enjoyed his association with Van Hise, especially in excursions which he made with him in conference on pre-Cambrian studies of the Appalachian Mountains. This brings to mind a day on the French Broad River, in North Carolina, where the two geologists invited the writer to accompany them, it being at the time his immediate field of work. The section examined comprised the base of the quartzites, which Keith has since determined to be Cambrian, and the underlying granites. There is a stretch of perhaps two hundred yards in which the obvious granite grades into the distinctly stratified quartzite, but in which no plane of contact can be recognized. After passing carefully over the section a couple of times together, the two separated.

Van Hise had begun to collect a series of specimens which should show the mineralogical character of the crystalline and sedimentary rocks and of the transition zone. He was thinking of the metamorphism, which according to hypothesis had altered an arkose sediment into something closely resembling the parent granite, and was considering the distinctive criteria which might be looked for under the microscope. He afterwards published papers on the secondary enlargement of crystals, in which these specimens played a part.

Pumpelly had turned to look at the tossing river and the beauties of the gorge. He spoke of the exhilaration of the scene, contrasted it with a desert landscape of granite hills and enveloping sands, which he made very real by a few graphic touches, outlined the process of disintegration which granite suffers under those conditions, and left it rather to be understood than stated, that the relations we had just been observing, in reminding him of the deserts of Arizona or central Asia, had suggested the aspects of the Appalachian region in a long past geologic age.

Each thinker followed his inherited lines of reasoning. Van Hise was adding a few precisely observed facts to that enormous mass of details upon which he based his thorough analysis of the most difficult problems of American geology, those of the pre-Cambrian. Pumpelly saw the facts, readily interpreted them in terms of known processes, drew an analogy with scenes he knew, and swept up on the wings of imagination to heights from which he could cast a glance down the long vista of the past.

It was with a penetrating understanding of the relation between sediments and the geographic or climatic conditions which govern their character and distribution that Pumpelly directed the investigation of the Green Mountains of New England. The general facts of stratigraphy and structure had been observed by the two Hitchcocks, Ebenezer, Emmons, Hall, J. D. Dana, and other contributors to the Taconic controversy. Each had espoused an idea and advocated it, but of precise observation and rational interpretation the very controversy itself had proved the need. Pumpelly took but little account of all that had been written. He went back to Nature's record. Having selected a critical area comprising Hoosac Mountain, an anticlinal ridge with a granitoid core, and Greylock Mountain, a synclinorium, he with his assistants, Wolff and Putnam, Dale and Hobbs, traced out in great detail the areal and structural relations of the granite, gneisses, schists, and limestones of which the geologic column is composed. Any careful observer might have done as much, but there remained several outstanding riddles that could be solved only by that use of the imagination which with Pumpelly was intuitive rather than deliberate.

Running through the sediments and gneisses and fading out into the granitoid rocks was an appearance of bedding which had been interpreted as such and had led to the opinion that the different rocks all belonged to a conformable series. Wolff and Dale, guided by Pumpelly, proved it to be a secondary schistosity, and thus disposed of the apparent conformity. The actual unconformity of the Cambrian sediments on the pre-Cambrian was then demonstrated by evidence of weathering of the older rocks in pre-Cambrian time, especially where a dike occurred in the granite, and the deposition of clastic sediments in the irregularities of the old land surface. Metamorphism had greatly obscured the original character of the sediments, some of which had been altered to gneisses, and Pumpelly credits Wolff with having carried out the tedious field and laboratory investigations required to establish the true relations. It may be noted that both chief and assistant were trained in exact

German methods, the one in mineralogy and structure, the other as a modern petrographer.

In the study of the Cambrian stratigraphy, which was obscured by schistocity, the true sequence of the strata could be ascertained only by the identification of obscure anticlinal or synclinal folds, where the dips are closely or actually isoclinal. Here the application of principles published by Heim in his "Mechanics of Mountain Building" was of critical value and Dale worked out a group of American examples of the laws originally recognized in Switzerland. These are today classical illustrations of the structures to be looked for in metamorphosed sediments.

A further problem presented itself in the differences of sequences of sedimentary beds in Mount Greylock and in Hoosac Mountain. In the former the Cambrian strata comprise both limestones and schists; in the latter there is only a great thickness of schists, both sections rising from the basal quartzite. The lack of agreement across the short span between the two ranges had been explained by the assumption of a fault, but Pumpelly and his assistants proved that there was no fault and solved the riddle by demonstrating that the strata in Hoosac Mountain are the near-shore equivalents of the offshore deposits which form Greylock.

It is the old story of Columbus's egg. The monograph on the Green Mountains has taken its place among the accepted results of geological research. The hypotheses have become part of the rationale of working geologists. We take for granted the penetrating insight into past geographic conditions, the understanding of obscure processes, the capacity for patient, conclusive investigation, those qualities which enabled Pumpelly to open a new era in the geology of New England and to settle a controversy which had lasted for half a century. It is as it should be. The perfect stone has found its place in the great structure of geologic science.

The elucidation of the problems of the Green Mountains was Pumpelly's last contribution to his adopted science, geology. In 1890, after six years of Government service, during which he had perforce given up the remunerative practice of his profession, he shared the lot of the scientists who work under official limitations. He was poor. The owner of large areas of iron lands, he saw their potential value being eaten up by taxes. Business conditions were such that there was little opportunity to recoup his fortunes through expert examinations of mining properties. He had a considerable family, led by himself in habits of lavish liberality. Under these adverse conditions, in 1893, he took his family abroad and during two years set his children an example of economy (so he says in

his "Reminiscences"). They traveled much in Italy, France, and Switzerland. One can imagine the mutual delight of their companionship among the scenes of the father's youth. Returning home in 1895, Pumpelly once more took up the practice of his profession, but, finding the opportunities few, he renewed his explorations of the Lake Superior iron ore districts. These efforts were, however, not particularly successful in the discovery of valuable ore deposits.

The years passed in home life among his growing children and in excursions to the Far West or to Mexico to make mine examinations, but in 1903, at the age of 66, he was once more seized with the desire to explore distant lands.

Explorations in Central Asia, 1903 to 1904

For forty years there had lain in Pumpelly's mind a spark, originally lighted by his imagination, which required but a breath to kindle it into flame. The central thought was the relation of climatic changes in central Asia to the migrations of peoples and the evolution of civilizations. In it were combined the fragmentary suggestions of Chinese history regarding ancient cities and peoples and the deductions from geographic and geologic evidence, which sufficed to give color to the hypothesis of a changing climate.

The spark was kindled by a fact recorded by an ancient Chinese mapmaker, who inscribed the words "Here dwell the Uşun, a people with red hair and blue eyes," on a map of the Tarim basin, in Chinese Turkestan, at a point apparently north of Kashgar. Another record mentioned the fact that many cities had been buried by advancing sand nearly two thousand years ago. These notes were brought to the attention of Pumpelly by the Chinese scholars employed by him during his stay in China in 1862-1863. He then felt that he was on the track of the ancestors of the European races (one can not help thinking how accurately the description fitted him, himself), and, pursuing a line of reasoning suggested by the diminishing inland lakes without outlets, he attributed the migrations of these peoples to a desiccation of the climate of central Asia. This hypothesis, since elaborated by Huntington, who was one of his assistants in 1903, lay for forty years in its author's mind, withheld from development by that scientific caution which had prevented him, when but a boy, from publishing his idea of an inland sea covering the Sahara.

The Chinese records and the facts he observed in his journey across Siberia in 1863 had suggested the idea of the former existence of an

inland sea in the now desert regions of Asia, and subsequent speculation had connected the sea with the Glacial Period, while its shrinkage would naturally follow from the waning of the conditions that had caused glaciation. But this logical chain of ideas lacked confirmatory evidence until Pumpelly was told by the Russian geologist, Tchernyscheff, that strata containing shells of the Glacial Period had been found in a position that seemed to indicate an inland sea of that time. The "dream," as he called it, then assumed the form of legitimate hypothesis, worthy of being tested. It had previously seemed to him through all those years, during which he had discussed it with scientific friends, too subjective in character.

Furnished with a grant of funds by the Carnegie Institution of Washington, Pumpelly carried out two expeditions to Turkestan, one in 1903, to reconnoiter the country and seek for the evidences of geologic changes in association with those of ancient civilizations; the other in 1904, to excavate old dwelling sites. In the organization of these expeditions he was guided by the experience of a lifetime of travel and aided by the prestige of the Institution he represented, as well as by his own great reputation. He chose as assistants specialists distinguished in physiography and archeology and, as was his custom, gave them free rein in their investigations, together with the fullest possible opportunity to publish their results as their own.

The purpose of the expeditions was chiefly ethnological, as the dream had been. In his youth Pumpelly had been divided in his interest between history and geology. Chance had directed the emphasis of his training to geology, but he had always shown a profound, if not a preferential, interest in his fellow-men and their development. Now the earth-science stepped into the background. He assigned to W. M. Davis, Ellsworth Huntington, and his son, Raphael W. Pumpelly, the studies of the physical basis of the human history, and, after giving them the outline of his hypothesis to prove or disprove, as the facts might decide, he turned to the archeological investigations with enthusiasm. In 1905, when he was entering upon the four years of preparation which intervened between the close of the field-work and the completion of his brief summary of results attained, he said to the writer: "I never could read books on geology; I had to see the facts to become interested, but I can read this by the hour," and he held up "Ripley's Races of Europe."

Very important contributions to our knowledge of the glacial and postglacial conditions which have obtained in central Asia were made by Davis, Huntington, and R. W. Pumpelly as a result of the expeditions.

They are fully set forth in the volume entitled "Explorations in Turkestan," issued by the Carnegie Institution of Washington, in 1905, as Publication Number 26. The archeological results of the second expedition appeared in 1908, under the same general title, as Publication Number 73.

Elected President of the Geological Society of America for 1905, Pumpelly chose for his address a subject drawn from his Asiatic investigations, "Interdependent evolution of oases and civilizations," and presented it at Ottawa in 1906.

Reviewing the field of study in central Asia after his return from the first reconnaissance, in 1904, Pumpelly wrote:

"Judging from our observations and from those of others, especially of the Arabian writers and of the later Russian explorers, it would seem that the country has long been an interior region, dependent for its life mainly on the snows and glaciers of the mountains; that there have been within the present geological period great fluctuations in the amount of water derived from the mountains, as recorded in the high and low shorelines of the seas and in the strata containing living forms left by different expansions of the united waters of the Aral and Caspian, and that man already existed within the region during at least the last great maximum of moisture. . . .

"While we have been surprised at the abundance of the data in natural and artificial records offered by the region, . . . we are impressed with a realization of the intimate relation in which this region stands to the Quaternary and prehistoric history of the whole continent. Physically it forms part of the great interior region extending from the Mediterranean to Manchuria, whose history had been one of progressive desiccation, but in Russian Turkestan the effects of this have been mitigated by the snows of the lofty ranges and the lower altitude of the plains.

"Archeologically this region has, through a long period, been a center of production and commerce, connecting the eastern, western, and southern nations, and its accumulating wealth has made it repeatedly the prey of invading armies. It has been from remote time the field of contact and contest between the Turanian and Aryan stocks; but its problems, both physical and archeological, are parts of the greater problem underlying the study of the development of man and his civilization on the great continent and of the environment conditioning that development."

Allowing Pumpelly to speak further regarding the last scientific work of his long and varied activities, we may quote from the preface to his final report on the work in Turkestan those paragraphs which show his attitude of mind toward his subject, toward his fellow-workers, and in regard to the realization of his dream. He says:

"While each of the investigators was expected to work up his material, there devolved upon me, as initiator and director of the expeditions, the duty of presenting an independent discussion of the results as a whole. I found

myself confronted with the task of translating and editing the contributions of the experts, and of drawing my own conclusions from these and from my own observations. To do this, I surrounded myself with a library of six hundred or more volumes related to our work and problems, besides many borrowed from libraries. Literally living in this problem for nearly four years, my whole time, reading, and thought have been devoted to acquiring such a general survey of the field as would enable me to discuss the subject of our results and of their wider bearing in the light of the present condition of archeological and ethnological knowledge.

"Besides incidental inspection of the museums of Tiflis and Tashkent, numerous visits for study were made to those of Moscow, Saint Petersburg, Berlin, Vienna, Zurich, Schafhausen, Cairo, Athens, London, Naples, and Rome, and to those of Paris, including M. de Morgan's systematically collected finds from Susiana—to me, perhaps the most important of all—and in connection with my chapter on chronology a special journey was made to Egypt to study the rate of growth of Egyptian village mounds in comparison with those of Anau.

"Of the two alternatives, confining the reports, my own included, to a record of observations and finds, or having each contributor go further and, treating his subject from the comparative point of view, draw his conclusions as to the bearing of his results on the general question of Eurasian problems, the latter seemed preferable; for, with the whole chain of observation and thought fresh in mind, it would seem to be the province of the individual investigator to state his inferences, even if only as working hypotheses.

"I confess to having written a chapter on the Aryan problem in the light of an extended study of the whole field and of our own results; but this I have suppressed, because it seemed a premature as well as a hazardous venture for one not already an authority on the subject . . .

"And now, what relation do the results bear to the dream that gave rise to the expeditions? On the physical side, Messrs. Davis, Huntington, and R. W. Pumpelly have traced in high Asia the records of several great glacial expansions during the Glacial Period. The climatic conditions, which during that period so greatly expanded these glaciers and buried Russia under thousands of feet of ice, presumably produced also the inland sea whose shorelines are still visible.

"The evolution of civilization has been traced backward to a time when, before its datings in Babylon and Egypt, man at Anau already lived in cities, cultivated wheat and barley, began the domestication of the useful animals which are our inheritance, and possessed the fundamental industrial arts, including a certain amount of metallurgical knowledge. Evidence has been traced of a progressive desiccation throughout long climatic cycles in whose favorable extremes civilizations flourished, which disappeared in the arid extremes. And that the climatic conditions under which these civilizations vanished gave rise to very early migrations and to a constructive reaction upon the outside world would seem to follow from the early appearance, in Babylonia and Egypt and in the late Stone Age in Europe, of wheat and barley and of breeds of domestic animals which Dr. Duerst identifies with those first established on the Transcaspian oases. . . .

"The reader will see that in tracing back to central Asia the source of the fundamental elements of western civilization, in finding the traces and causes of the inland sea, in discovering evidence of progressive desiccation (and in this the cause of the migrations that revolutionized the world), the dream has to this extent been realized."

This was written at his summer home in Dublin, New Hampshire, in 1908. Might not the old man, who had cherished that dream for forty-four years, have with justice put his statement of its realization more strongly? Might not one who knew his exuberant vitality, his optimism, have expected that he would? But no, his was fundamentally a scientific mind, adhering above all things to the truth as he understood it, and no impulse of self-gratulation could urge him to surpass its limits. In the suppression of his own chapter on the Aryan problem, the ultimate speculation to which he had given years of thought, we see the expression of that supreme loyalty to his ideal of truth which made him truly great.

Dublin, New Hampshire

The last dozen years of Pumpelly's long life were passed at his homes in Newport, Rhode Island, and Dublin, New Hampshire, alternating summer and winter, except when making journeys abroad or in this country for the pleasure and education of his children. Mrs. Pumpelly died in 1915, after forty-six years of married life in a relation more complete in its harmony than falls to the lot of but few. In his loneliness, Pumpelly drew even nearer, if possible, to his children and with them turned to the desert, where "Blessed are the realms of Silence, for in them is the nearness of God." He revisited the scenes of his labors and marvelous escape from Apaches and Mexicans and again plunged into the waterless wastes in search of the Old Yuma Trail, along which he had ridden fifty-four years before. The Ford cars proving less able to cope with the sand than horses had been and the water giving out, the desert almost claimed him permanently as its own; but, with his habitual resourcefulness and the guidance of an Indian as old as he himself, he once more escaped.

Of his life and influence at Dublin, his old classmate and intimate friend, Henry Holt, has written:

"It was most exceptional and it was all unconscious—simply the action and reaction between his character and that of his neighbors. Probably never elsewhere was such a community. He was incomparably the most influential person in the place—ruled it without knowing that he did—unconsciously attracted there all forms of excellence and unconsciously repelled any form

of pettiness. The circle he drew around him blended the highest aristocracy with the simplest democracy. A visitor once described it to a stranger: 'One night you'll go to as lovely a ball as you ever saw, and the next night you'll dine with people you met there who do their own work.' That realization of Utopia those who shared and marveled at it knew was the involuntary work of Pumpelly. He loved all people worth loving and had no other standards; and all people worth loving loved him."

There is nothing to add to this statement of the humanity of this remarkable man. His great soul possessed the magnet of love for his fellow-men, without distinction of creed or race, excluding only the false. His penetrating intellect threw the searchlight beams of his imagination into the unknown realms of knowledge in search of Truth. To her he was ever absolutely loyal, never allowing even a suspicion of egotism to color a statement of fact or to influence his estimate of the validity of an hypothesis. To the Truth he was true. Therefore his contributions to his adopted science, geology, stand unchallenged, monuments to its progress, while he himself takes his place outside the circle of specialists, in the group of great explorers of the physical and intellectual worlds. His thought is being handed down, a living, growing influence, by many who are as unconscious as its author often was of its intense vitality. It lives on because its author invariably put Truth before self.

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Franklin P. Mall.

Recent Portrait of Dr. Franklin Paine Mall
By
Thomas C. Corner
(From a Photograph Taken in 1913 by the Late Dr. Frederick L. Gates)

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

BIOGRAPHICAL MEMOIRS

VOLUME XVI—THIRD MEMOIR

BIOGRAPHICAL MEMOIR

OF

FRANKLIN PAINE MALL

1862-1917

BY

FLORENCE R. SABIN

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1934

FRANKLIN PAINE MALL

1862-1917

BY FLORENCE R. SABIN

Seventeen years have elapsed since the death of Franklin Paine Mall, and the interval between his death and this belated report to the National Academy of Sciences but serves to make one the more aware of the power of the man and the enduring influence of his life.* Little known to the public then or now, never heralded as a brilliant discoverer, one has but to mention his name to his colleagues to hear that he was the leader of them all.

In Mall's files was a note from P. A. Levene, in which is condensed into one sentence what one must reveal to paint Mall's portrait: "The few times I have met you are a radiant memory", and such he remains to those who knew him. Mall must be described as a man of power, yet he was modest, unassuming, even shy. Physically he was unimpressive, thin, of average height and sallow complexion. His face, when not lighted by a smile, was apt to be sad. Always youthful in appearance, he was often mistaken for a student, yet this frail young man was one of the outstanding educators of his time. In the days when scientific research in medicine was taking root in this country, he was one who planted and nurtured it. He found that the ancient science of anatomy was only a handmaid to surgery in this country, with practically no independent status, and he left it in its rightful place with a school of disciples. To this day there is no dearth of well trained, scientific anatomists, the second and third generation from Mall. Moreover, his influence was felt throughout medical education, in the founding of journals and scientific societies, in that period of awakening which began in this country in the last decades of the nineteenth century.

The story of Mall is of a mind brilliant and original, with a strange enigmatic quality and a complex combination of opposites—wit and wisdom, humor and sadness. One might easily insist that raillery or a certain acid quality of his comment was his most striking characteristic, but another would combat this with surprise and substitute kindliness and judgment. But all

*A more extended life of Doctor Mall will be published by the Johns Hopkins Press.

were aware of his tendency to say the unexpected and of an extraordinarily cryptic quality of his mind, which acted like an enzyme or ferment on the minds of others, goading them to thought and action. In this lay his power as a teacher, combined with the example of an enduring enthusiasm for study. Endowed with exceptional ability for organization, in a country where this power receives the highest recognition and rewards, he continued to lead the simple life of the scholar, as active in research at the time of his death as in the early years of his training. It is interesting to note that he had no ability as a lecturer and left no popular writing whatever, yet no medical student escapes his influence.

He was a leader in the sense of one who foresees the trends of development of his times and sets the stage for their fulfillment. He can best be described as having the abilities of generalship. He knew exactly what he wanted; he could plan his campaign for years in advance and could wait with patience until the time was ripe for the next step in his program. He could bend the will of others to the interests of causes. Of William Welch he once said to me that no one had ever found him working for his own advancement, and the same was true of Mall. His interests were wholly objective; he was a fighter for freedom in education; an ardent democrat, he knew the elements of danger in democracy and knew how to combat its leveling tendencies. His vision was of the development in this country of a university of scholars in the European sense; and medical education is on a higher level because he lived and taught. In the medical school which developed in Baltimore, he and Osler represented two different phases of thought, a difference which Osler has expressed better than anyone else can do, for he said that he himself was of the type who would rather be wrong with Plato than right with Aristotle. As in ancient days, two types still make up the medical profession. It is clear enough to which Mall belonged for the motive of his life was a profound belief in the benefits of the growing science of medicine; he had unbounded faith in the ability of man's mind to solve the problems of disease.

Franklin Paine Mall was born in Belle Plaine, Iowa, September 28, 1862, and died in Baltimore, November 17, 1917, at the age of fifty-five, following an operation for gallstones. He was of German parentage on both sides of the family. His father, Franz Mall, was born in Söllingen, near Durlach, in the Province of Baden, Germany. Mall's branch of the family has lived there for three hundred years.[1]

There are two strains of the Mall family in Germany: one, descendants of Tobias Mall, who was born in 1570 and lived in Donnstetten, and the other, the line from which Mall came, descendants of Hans Wendel Mall, a linen weaver, born in 1625, who lived in Söllingen. Before this time the name of Mall, meaning "hammer", does not appear in the records of the town. It was the period of the thirty years' war, during the early years of which Hans Wendel Mall was born. The region of Durlach was invaded; at the beginning of the war, Söllingen had 750 inhabitants, and at the end only 250, which tells the tale of Hans Mall's youth. The records of births and deaths were kept by the church, and though all of the church books of the neighboring towns were destroyed during the war, there is a church record of baptisms in Söllingen which begins May 8, 1614. Further back than this it is difficult to trace the name of Mall, but it is probable that the family originally stemmed from the Tyrol. The record that Johann Erhard Mall married a Papist, which I found in a note left by Mall's father, indicates that the family was Protestant.

Mall's grandfather had seven sons, six of whom emigrated to America. Mall's father was a boy of nineteen at the time of the revolution of 1848. He belonged to a choral society of twenty-four young men and during the revolution some of their songs, referring to liberty, so displeased the authorities that all

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[1] For the data in regard to Mall's family in Germany, I am indebted to Mr. Daniel Mall of Württemberg, Germany, a second cousin of Dr. Mall. In part the data are from letters from Mr. Mall and in part from his book, *Das Geschlecht der Mall*, published by Hugo Kretschmer, Kunstanstalt für Hoch- und Flachdruck, Görlitz. Volume I in 1925; Volume II in 1926.

twenty-four served a term in prison. The following years were a period of unrest in Germany, times were hard and there was a shortage of food. In 1852 Mall's father and one of his friends, Joseph Wenz, went to America in a sailing vessel, being thirty-six days on the water. They joined the trek westward, finally reaching St. Louis, whence they went on foot to Iowa where they had heard of a chance to buy land. Franz Mall bought a tract of land there which later became his farm. In two years, however, he had news that his father was ill with gastric cancer, and he returned to Söllingen. He seems to have been a leader among his companions for when he returned to America that same year, 1854, all of his choral society came with him. Moreover, through him five of his brothers came to America and settled on farms in Iowa and Kansas. In 1855, the year after his return to America, he married Louise Christine Miller of Oswego, Illinois. Little is known of her except that she was born in Germany. There were four children, only three of whom grew up—two daughters and one son. Mall's father must have been a well-to-do farmer, for he was able to give his son an education that took years, and he himself returned to Germany four times and finally died in Söllingen. To those who remained in Germany the brothers who went to America must have seemed rich for in the town of Söllingen the family were all small farmers. None of them had more than about twenty-five acres of land, divided as was the custom in Germany into small plots of from a half to one and a half acres. The value of these small plots of land depended not only on the nature of the soil but on the nearness to the village. Since these holdings of land were not enough for the support of a family, each man had some other occupation such as a grafter of trees, stone cutter, quarryman, mason, worker on the railroad and cultivator of vineyards. The records state that none of the Malls had wealth, but that as a family they stood out in the community for honesty and for idealism. In these two qualities it is clear enough that Mall was representative of his family, but there is little to indicate that Mall was interested in tracing his ancestry or in handing down the knowledge to his children. However, he did

tell his daughter Margaret that there had been a Margaret Mall in the family for several generations and that one of them, a cousin, was an able teacher. Up to the time of Franklin Mall, none of the family had ever had a university education; but since his time three of the Malls of Söllingen have studied medicine and in America, several of the family have had advanced education, including both of Mall's daughters.

Mall's mother died when he was ten; his father married a second time and, as the stepmother was unsympathetic toward the boy Frank, an elder sister took his mother's place in his affections. Mall was evidently a difficult child; he was clearly unhappy; neither at home nor in school did he find outlet for his active mind, and the whimsical humor of his later life was shown in endless teasing. It was a teacher in a local academy, a John McCarthy, who changed the whole tenor of his attitude, as is shown in a few words from one of Mall's letters to a nephew who is a teacher of history: "Your schedule of work looks very interesting and it must be very interesting to you to give it. My avocation has been a study of history, especially that of the Renaissance. If I had not gone into anatomy, I might have taken up history with equal enthusiasm. When I was a boy I detested history, but good old Mr. McCarthy showed me that I liked it. I only detested the detestable way of teaching it by rote." These words, which paint the picture of the boy, Frank Mall, make one certain that he made the teachers of the "detestable way" fairly unhappy in their turn. The intensity of his own reaction was shown years later in his determination to reform the methods of teaching science by rote. On this boy the influence of McCarthy was profound and in Mall's files is a treasured letter from him in his old age: "I keep up the study in a stumbling way, but when I am ready to perish from lack of knowledge my help comes from those wonderfully kind and modest managers of observatories. I am more contented when doing a little studying. I would love to teach, if I could only hear well enough, but that day is over." This letter reveals the secret of Mall's start in life and it tells as well the whole theory

of teaching. Mall met a teacher in an obscure academy in Iowa, who loved study and could awaken that spirit in others.

Our recent interest in the history of the settlement of the west in our country makes it possible to picture a small town in Iowa in the years following the Civil War—a farming community of English and German people, pioneers of English stock from New England, and forty-eighters directly from Germany. In Mall's home one can estimate that there was relatively little culture, for his early style in writing does not show any familiarity with good literature. It was a time when few homes had many books but when more than one of our public men had a style clearly formed from the Bible and Shakespeare. I well remember the keen interest with which Mall in his mature years read the Bible for the first time and his amazement at its beauty, for he read it as literature with an appreciation not dulled by any familiarity with the text. Likewise he read Shakespeare and the German classics for the first time in his student days in Germany. Nevertheless Mall's father had the traditional German respect for learning, for Mall wrote of him later: "I have been unusually fortunate in having a father who believed in education." Thus, after McCarthy had shown the boy Frank Mall that he loved study, Mall's father, though with slender means, helped him to get an education.

At the age of 18, Mall entered the Medical Department of the University of Michigan. There is no record whatever as to what turned his mind toward medicine, but it is said that the local physician had studied at Ann Arbor. It was a period when Michigan University was clearly making progress in education. The year Mall entered, 1880, the standards of admission were raised. The records show that no student was admitted to the Medical School under the age of 16, and that every candidate, who could not show a certificate of graduation from a respectable high school, academy or college, had to submit to an examination in the elementary branches of an English education. This now seems meager enough but it was a real advance in medical education in this country, for as President Eliot of Harvard University has said, "In the Sixties and Seventies there were no

requirements for admission to our medical schools. To secure admission a young man had nothing to do but to register his name and pay a fee." In 1880 the medical course at Ann Arbor was graded and for the first time in any institution in this country raised to three years. Many years later, from the standpoint of his mature judgment, Mall wrote of these changes in Michigan University as follows:

"Twenty years ago the medical department of the University was just beginning the transformation which has changed it from a school of low standard, as were all medical schools at that time, to one of high standard as it is at present. Requirements for admission had just been introduced, and a graded course of study extending through three college years was made compulsory for all students. During this transformation it was necessary to introduce much new work both in the laboratory and in the lecture hall to occupy the students during the increased time they now had to study medicine. Old instructors filled their share of the additional time by requiring the students to attend their lectures a second time in order that they might remember better the many facts presented to them. Yet there was considerable vacant time to be filled, which thus gave an opportunity to new instructors to come to the front with new ideals and methods to enrich the medical course. It was this second group of instructors, whose ideals and methods are now generally acceptable, that made the greatest impression upon me. Foremost among them were Professors Victor C. Vaughan and Henry Sewall. I can remember well the first lecture of each of these men. They entered upon their work by giving out matter first hand, and from the beginning made the impression that they mastered their respective subjects. They dealt little with the opinions of others, but instead produced trustworthy facts and demonstrations, as well as laboratory experiments for the students, upon which to build. The principle involved appeared to be the development of the student while presenting the subject matter, and now it is plain to me that no one but an investigator in his subject can do this.

"These high ideals were shared to a greater or less extent by other instructors, and were acceptable, it appeared to me, to only a minority of the students. The majority of students were seeking a certain quantity of knowledge, and preferred to have it drilled into them. Little did the solving of problems and the development of reason appeal to them, and it naturally followed that they mistook versatility for power. An educational

institution of highest order must carry on perpetual warfare against drilling trades into inferior students, in order to retain its high position. And above all the medical profession should be filled with learned men, and not tradesmen, in order to be of the greatest good to the community. It appears to me that the change beginning to take place in the medical department in 1880 was towards training thinking physicians with an underlying foundation composed of recent medical research. In other words its goal was toward the university stature. At this time, however, the department was yet only of high-school stature, and university ideals certainly seemed much out of place. But its rise has been very rapid, for it is now through its college stage and is about to enter its true university career.

"It is then the force which marked the beginning of the university ideal in the medical department at Ann Arbor which I remember best. This force which encouraged thinking and investigating has been carried from the University by many of its graduates, and has always proved to be a trustworthy friend. Fortunately for the medical department as well as for medicine of the entire land, this force has been carefully guarded and cultivated by our present distinguished dean."

After graduation from Michigan University, Mall went to Germany to study. The influences which sent him abroad must be surmised. While for a hundred years men from the eastern seaboard had been going abroad, first to Paris and later to Edinburgh to study medicine, it was probably a rare thing for any of the early settlers of the west to do so. Dr. Henry Sewall, just referred to, who went to Michigan in physiology in the spring of the year Mall graduated, had studied under Sir Michael Foster in Cambridge and under Kühne and Ludwig in Germany and had taught under Newell Martin in the Johns Hopkins University, before the Medical School was started in Baltimore. There is a story that Sewall, who had given the seniors a few lectures, asked the Dean of the School what he should do about an examination, with the result that the four best students of the class were sent to him for a test—Frank Mall, Will Mayo and two others. They all failed and Sewall is said to have predicted that none of them would succeed either in science or in medicine. It is clear enough that this episode did not delay graduation for any of them, nor did it dull Mall's appreciation

of Sewall, but its immediate effect on him is an interesting speculation. Did failure at Michigan send Mall to Europe and did it spell success at Leipzig? At any rate Michigan did not introduce Mall into scientific medicine, for he went to Europe for clinical work rather than for research, for Dr. William H. Welch, who first met Mall in Ludwig's laboratory in Leipzig, has written to me: "Mall went to Germany for clinical work, as so many young doctors were accustomed to do and without any particular interest in science. It was his contact with His and later with Ludwig which opened his eyes to the sciences of anatomy and physiology. Ludwig, with whom I had worked in 1876-77 at the time of my first two years in Germany and who greatly influenced me by turning me to Cohnheim instead of to Virchow, used to love to talk with me about Mall, whom he credited with remarkable gifts of observation, '*Raumsinn*,' he called it."

The first year in Germany Mall spent in Heidelberg working mainly on the nervous system and on the eye, both in the laboratory and in clinics, with the intention of going into ophthalmology. It was, however, the golden age of the German university and here Mall passed directly from what he had termed the medical high school into the freedom of university life. Here he found students who were mature men, better trained than he, and planning their own education. This he was keen to do, and his notebooks show that he planned his own course and worked with tremendous energy. He used to say that he enjoyed the chance to study pathology before he had taken a course in histology and it is clear that he found out for himself that he wanted to try research. Perhaps the same thing happened to him that history shows had happened to von Baer sixty years earlier, namely that a great dissatisfaction with medicine as it stood came over him and made him determine to find out what research was like. At any rate, in the fall of 1884, Mall went to Leipzig and started in research under His. Mall was interested in structure and His was the most outstanding anatomist of his day. Mall knocked several times at his door before he was finally admitted. His did not seek students to work with him; he was not interested in beginners. But something in the per-

sistence of that spare, young American of twenty-two years must have impressed the professor, for he finally gave Mall a place in his laboratory.

The history of Mall's first problem under His reveals at once Mall's ability. Inexperienced in research, a novice in embryology, he made acute observations, clarified an obscure field, and came to conclusions directly opposed to those of His. There thus arose between the two men, one, an experienced professor of years, the other a young student with his first problem, a scientific controversy rarely surpassed in its high plane of intellectual fairness. In retrospect one could not wish a word of it changed.

His suggested to Mall that he study the development of the gill arches in the chick and the results of his work focused attention on the question of the origin of the thymus. Shortly after Rathke [2] (1825) had discovered the occurrence of gill clefts in mammalian embryos, the subject of the origin of the thyroid and thymus became active problems. When one reflects on the crudeness of the early methods, without serial sections or any form of reconstruction, it is not difficult to understand why differences of opinion arose. It is interesting to note that it was in connection with the active discussions concerning the origin of these glands that Born [3] devised his method of reconstruction by wax plates, and that the first results of this method were to show that there are both median and lateral primordia of the thyroid, and that the thymus arises in the third branchial cleft. The most important of the earlier studies on the thymus were those of Remak [4] who formed the view that the thymus arose from the endoderm of the pharynx. But this view, now believed correct, was soon replaced even in Remak's mind by the theory that the thymus was mesodermal in origin, an opinion

[2] Rathke, M. H., *Isis* von Oken, 1825, 747-749, 1100-1101.

[3] Born, G., Die Plattenmodelirmethode, *Archiv. für mikroscopische Anatomie,* 1883, *22,* 584.

[4] Remak, R., Untersuchungen über die Entwickelung der Wirbelthiere, Berlin, G. Reimer, 1851 [1855].

which obtained until Kölliker[5] and His[6] both held that it came from epithelium. In the study of human embryos with the methods of serial sections which he had perfected, His believed that the thymus arose from the ectoderm of the third gill cleft, along the wall of a deep groove which he called the sinus praecervicalis. Mall found evidence, on the other hand, that the thymus came from the endoderm of the pharynx. It is clear that the matter was not much discussed by the two men while Mall was at work, for it was the theory of the older man that a research worker should be wholly independent while his work was in progress. As Mall has related, when His took a student in research, he outlined a problem, explained and demonstrated the methods, and then was annoyed if he was consulted over the details. Thus at the end of the year, Mall told His of the results of the work and gave him his paper written in English. Under the advice of His, Mall had applied to work in Ludwig's laboratory and during the next winter Mall restudied the development of the thymus, coming, however, to the same conclusions as at first, and thus he returned the paper to His, now translated into German. This time it was accepted, and, to follow its fate, we pass over, for the moment, the momentous year which Mall spent with Ludwig.

Mall left Germany in the summer of 1886, to become Fellow in Pathology under Welch in Baltimore. During the year which followed, before his paper appeared in the *Archiv für Anatomie und Entwicklungsgeschichte,* of which His was editor, there was much interesting correspondence between the two men. His studied Mall's paper carefully, selected the best of the drawings, and had them copied on lithographic plates, concerning which Mall wrote to His, February 23, 1887: "A few days ago I received the plates and could hardly believe that they represented the rough work I gave you last autumn. I hardly know how to express my gratefulness to you." His then wrote that he was

[5] Kölliker, A., Entwicklungsgeschichte des Menschen und der höheren Thiere, Leipzig, W. Engelmann, 1861.

[6] His, W., Anatomie menschlicher Embryonen. 3 vols., Leipzig, F. C. W. Vogel, 1880-1885.

reworking the subject in new human embryos, to which Mall replied: "I am glad to hear that, in your reworking of the thymus, you find again that it arises from the ectoderm." In this statement Mall was entirely sincere, for his respect for the elder man was so great that emotionally he would have been content to have His correct, but he added at once: "From my material, I cannot see how it is possible. My embryo shows definitely whence the thymus comes. In the near future I hope to take up the subject in the dog. If I have been mistaken in the chick, it is my duty to find it out and acknowledge it."

It is interesting to note with what courtesy and deference he addressed the older man, using always the title of "Honored Professor" and concluding his letters with "Your grateful pupil", and yet how clearly and positively he expressed his own views. His wrote that in the correction of Mall's paper he had been exceedingly careful not to change Mall's meaning, but that he had omitted considerable of the data since the reader wishes only to get a clear picture of the chief points and not to follow through the entire material. The clear-cut statement of Mall's difference from his own views was not deleted and in the printed article is as follows (1, Page 18):

"This nodule is a round or oval body which His, in his study of the chick, has described as the beginning of the thyroid and Seessel as the parathyroid; but, as a matter of fact, as will be shown later, it is the thymus."

These were the words of the young man of twenty-two, working under the direction of a recognized master, to whom he gave in the same paper the following significant expression of his gratitude:

"This work was done under the direction of Herrn Prof. Dr. His, to whom I am extraordinarily indebted, not only for having been allowed to work with unlimited freedom in his laboratory, but also for instruction in methods, and for the expression of his views on various points during the progress of the work."

In the number of the journal preceding the one in which Mall's paper appeared, His published again on the thymus, strengthening his own opinion, and later Mall's paper was accompanied

by a footnote by His, explaining the delay in publication and adding: "The work gives evidence, moreover, as one can easily note, of its completely independent character."

When Mall first saw his paper in print, he wrote:

"A few days ago I saw the new number of the *Archiv für Anatomie und Entwicklungsgeschichte* and was gratified to see how much better my article appeared than when I handed it over to you. . . . You do not know how grateful I am to you for all the trouble you have put yourself to in going through my terrible article. I know that I shall profit by it. At least I shall try to follow the example you have set." Then immediately he dropped the rôle of the beginner and set forth his views as a mature and experienced worker. In a brief paragraph he analyzed the whole matter. He had now a new series of dog embryos and he had found the small epithelial structure which His had taken for the thymus was a transitory organ which joined the vagus and thus could not be the thymus, or, in Mall's own words: "But I am fully convinced that you have erred regarding the thymus and the body you have described as such is an organ of the vagus." This organ is now known as one of the placodes, or the homologue of the lateral line sense organs of fishes.

Mall then published three papers, two of them from material he had prepared in Leipzig. The first was on the development of the Eustachian tube in the chick (5), the second on the first branchial cleft in the chick (3), and the third on the branchial clefts in the dog (6). In these papers he described in detail the placodes, to which he had referred in the letter to His (3, 4, 5, 6). The union of the ganglion of the facial with the ectoderm was first seen by van Wijhe[7] in 1882, then by Froriep,[8] by Beard,[9] and then by Mall, in 1888 (6). In this paper Mall made it clear that the corresponding body (placode) which joined the vagus

[7] van Wijhe, J. W., Ueber die Mesodermsegmente und die Entwicklung der Nerven des Selachierkopfes, Amsterdam, 1882.

[8] Froriep, A., *Archiv für Anatomie und Entwickelungsgeschichte,* 1885, 1.

[9] Beard, J., Zoölogischer Anzeiger, 1884, 7, 123-126, 140-143.

had nothing to do with the formation of the thymus gland, for he said (6, page 211):

"Can it now be possible that the organ from the third pocket comes and disappears, and that the organ of the vagus leaves the vagus and forms the thymus? Prof. His' error no doubt lies in the fact that the sinus præcervicalis is long in developing. It forms a very conspicuous body in the region of the thymus. Just as this body from the sinus præcervicalis is disappearing, the thymus rapidly appears and takes its place."

Thus again one see the decisiveness with which Mall expressed his differences with his former teacher, both in letters to him and in print, so that there was no ambiguity either as regards Mall's views on the problem or concerning his profound respect for and gratitude to His. When he sent these reprints to Prof. His, he wrote: "Today I send you two reprints of some work, partly done with you and partly done by myself during last summer's vacation. I am really sorry that the work turned out as it did, for I should much rather have been able to retract my former statement regarding the thymus than to confirm it. I hope that you will not find these pages worthless."

The answer to this letter must have been exciting to Mall, for His had obtained some new human embryos and in reworking the subject he had found that Mall had been right—that is, he now saw that the thymus arises from the lining of the pharynx. He said, however, that there might be a small addition to the organ from ectoderm, and it is probable that His never appreciated the force of the evidence concerning the placodes, as Mall indicated later in his final reviews of the subject (17, 51). His[10] made the correction of this matter in the form of an open letter to Mall published in the same journal in which their articles had appeared.

This letter from His, as well as the correction in print, must have been a gratification to Mall, for it is but human to rejoice when one's work has been proved correct. But the reply again

[10] His, W., Schlundspalten und Thymusanlage. (Aus einem Briefe von W. His an F. Mall in Baltimore), *Archiv für Anatomie und Entwickelungsgeschichte,* 1889, 155.

shows an ideal relation between two scientific workers, and only in the greeting does one sense the difference in age.

"Honored Professor:

"In one sense of the term I am grateful that our controversy is over, for ever since my conclusion was so different from yours, I have not felt any too well over the subject. It would have been far more agreeable to me had I found that my view was incorrect.

"Regarding the possibility of the ectoderm aiding to form the thymus, I see no good reason why it should be so. The concentric corpuscles do represent the pathological changes that take place in the skin, but a new formation from the larynx or from the esophagus shows the same changes. If I do not mistake you, you compare the concentric corpuscles to the pearls of epitheliomata, and since pearls are formed in cancers of the alimentary canal above the stomach, endoderm may as well form concentric corpuscles as ectoderm. On the other hand, first, amphibian thymus glands have no concentric corpuscles, while in higher vertebrates (dog, cat) the thymus is frequently filled with cysts lined with ciliated epithelial cells. This fact would argue in favor of endoderm.

"The argument that makes it highly improbable to me that the ectoderm does participate in the formation of the thymus is that, as I believe, the sinus præcervicalis is the 'branchial sense organ' of the vagus. It does not seem possible that an involution of ectoderm should partly blend with a nerve and partly with a gland.

"In my work I am still engaged with connective tissue. Among a few new things, I find that the bone is quite full of elastic fibrils. Also, I find that striated muscle behaves quite differently at the tendon than has formerly been described. I send you a sketch of such a muscle ending which is obtained by boiling in dilute acetic acid. I also send you two poor photographs of a reconstruction of the stomach.

"Otherwise I am getting along nicely, only that I have to teach (?) somewhat, for I have been appointed assistant. Most of my time I have for myself. Last spring I worked through the development of the frog. This winter, with the aid of the Government, I hope to work the development of the codfish and to pay especial attention to the blood vessels.

"I am looking forward to going to Leipzig for a few months next summer. This has been my very strong desire for the last two years.

"With best wishes,

Very gratefully, (Signed) F. P. MALL."

The history of Mall's first problem revealed his ability. It is clear that he went to Leipzig without preliminary training in embryology, that in a brief year he acquired the technique of serial sections, and then, by the new methods of reconstruction, he mastered the complex forms of the gill arch region in the embryo chick and put his finger on the crux of the differences of opinion regarding the origin of the thymus. He saw that the transitory ectodermal placodes, then just beginning to be recognized by embryologists and now well-known as the homologues of the lateral line sense organs of fishes, were the cause of the confusion, for the one which joined the vagus was close to the position of the future primordium of the thymus. In two years Mall had established the endodermal origin of the thymus gland and his final summing up of the subject, published only a few years later, 1893, (17) in the chapters on the origin of thymus and thyroid for Buck's Reference Handbook of Medical Sciences, is a clear description of a complex region. Following an account of the same region in fish embryos, he said: "In mammals the condition is much simpler. The branchial grooves lie on the side of the body, are shallow on their dorsal side and deep on their ventral. As these arches fall over one another, the grooves, as well as the third and fourth arches, are buried in the side of the neck. While this is taking place, a pit is first formed, the sinus præcervicalis of His. From the dorsal side of the first groove an invagination unites with the ganglia of the fifth nerve; from the second, the invagination is to the ninth nerve; and from the third and fourth it is to the tenth nerve. A section through these organs in the region of the vagus and of the thymus is shown in Fig. 627." (This figure from 17 is reproduced here.) "The ectodermal invagination is absolutely blended with the vagus and is only in apposition with the thymus."

The simple woodcut of this figure gives such convincing evidence of the statement above as to explain fully the decisiveness of Mall's conclusions on the origin of the thymus. These articles in the Reference Handbook are still the best articles yet written on the embryology of the gill arch region. To the medical historian they might serve as models for a study of the growth of

medical thought, for they show how problems arise, how they depend on the development of methods, and how they are based

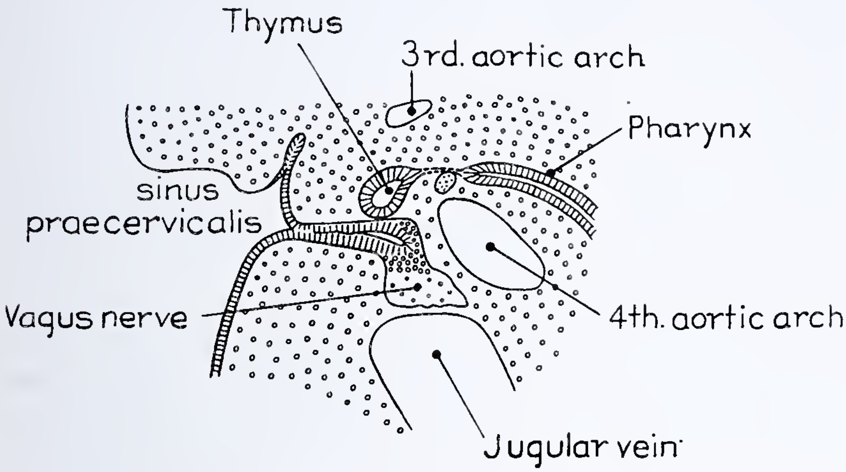

FIG. 1. (Fig. 627 in 17.) Section through the Thymus and Fundus Præcervicalis of a Dog's Embryo 10 mm. long: X 65. It is seen that the thymus is still in connection with the pharynx.

on previous work. To the student of Mall's development they reveal how profound a mastery of the work of the earlier embryologists he acquired in the laboratory of Professor His, and they show also his own originality and his independence of thought.

In October of 1885, Mall entered the laboratory of Professor Carl Ludwig. Just as had happened to him when he applied to work with His, he was not received at first, in this case because Ludwig's laboratory was always full of students and there was no room. By a fortunate chance, however, one of Ludwig's students left and, perhaps at the suggestion of His, Mall was offered this place; indeed, Mall's trunk was already on the way to the station when the invitation came. Thus began an influence which was the most dominating in Mall's life.

Ludwig suggested to Mall the study of the blood vessels and lymphatics of the small intestine, a problem in structure, but conducted without that artificial separation which certain techniques, such as the kymograph of Ludwig and the microtome of His, were soon to bring between physiology and anatomy. Under

the guidance of Ludwig, Mall learned the methods of injections of blood vessels and of lymphatics and made the specimens from which the well-known beautiful lithographic plates of his article were made. The brief introduction to this article, stating that while the study of the blood vessels of an organ could not give evidence of blood pressure or rate of blood flow, nevertheless, a complete picture of the pathways for the blood and lymph throughout an organ was essential for an understanding of the distribution of the force of the blood stream, shows plainly enough the guidance of the work along physiological lines, for which Mall was indebted to Ludwig.

In this study Mall not only got a concept of the entire vascular system of one organ, but he was able to make a reconstruction which enables one to visualize how all of the functioning tissues of that organ, the muscle coats and the mucosa, get their blood supply. I think that there is evidence that this was Mall's own contribution to the problem, for it was this which made Ludwig say of him that he had remarkable sense of structure in three dimensions—*"Raumsinn,"* as he call it. To gain this concept, Mall applied the methods of maceration and pulled the intestine apart into all of its various layers, so that he saw each layer of the organ in its relation to the rest; in other words, he took the intestine apart as a mechanic does an engine, so that from his own experience he knew the properties of each coat as a background for the study of its blood supply.

In the study of the complex vascular pattern he saw that the blood vessels of the intestine had developed in such a way as to give every segment of the organ an equal supply of blood and that the pattern of these vessels, complex as it might seem, could really be expressed as a simple branching of one artery into five different orders. Subsequently he was to show that this simple concept of five different orders of arteries for an organ had a general application to the vascular supply of all organs. His reconstruction made it easy to follow this pattern; the single superior mesenteric artery, with its rays of mesenteric branches, as vessels of the second order, and their anastomosing loops near the wall of the intestine, is easily seen and had, of course, long

been known. From these loops, long and short arteries, vessels of the third order entered the submucosa, giving rise there to the fourth order of distributing vessels, with a second series of anastomoses which completed the pattern for equalization of the blood flow. From the plexus of the fourth order, the mucosal vessels became broken up into the capillary bed around the crypts and under the epithelium of the villi. From this study, Mall got the concept of structural units of organs, using the villus of the intestine as the type. Thus the villus, with its single artery and capillary bed which it supplies, represents a final unit of function for which all of the various coats of the intestine are adapted.

In like manner Mall found that the lymphatics of the intestinal wall may be injected, if the needle enters the right level. The difficulty of lymphatic injections in the adult animal is due to the abundance of the valves. For the intestine, an injection of the central lacteals can be made only when the needle penetrates into the plexus at the base of the crypts, since there are no valves between these vessels and the central lacteals; while the two main plexuses, the one in the submucosa and the other between the two layers of the muscle, are demonstrated when the needle enters these two levels.

This study of the walls of the intestine, with the picture of vascular and lymphatic supply, has so completely entered into the body of our knowledge that one can say without exaggeration that no one studies the function of this organ without reference to Mall's work.

The years with His and Ludwig had been far more to Mall than just guidance in scientific problems, for the whole range of scientific thought and of general culture had been opened up to him. Especially in the year with His, he must have read a prodigious amount and it is clear that he had been led to follow the scientific work of the master minds. Also he attended many clinics, he followed cases to autopsy and studied the pathological sections. His letters home showed that art and literature, the drama and music had come into his ken for the first time; he wrote to his sister to read Shakespeare, not once but a dozen times and then its beauty would begin to appear. Likewise he

mastered Goethe as it can be done only in Germany with the aid of the theatre as well as by reading. Between him and Ludwig there developed a rare friendship, and in the companionship of these two men, both in the laboratory and in Ludwig's library at home, there was revealed to the younger man an idealism and a devotion to scientific research which was to mean so much to the development of American medicine. When Mall said good-bye to Ludwig, and tried to thank him for all he had done for him, Ludwig said to him, "If you feel this way about it, pass it on." This is the key to Mall's devotion to the cause of research and to the teachings of this master Mall was faithful unto death. None of Mall's letters to Ludwig has been kept, but the following one from Ludwig to Mall shows what the worship of Nature meant to Ludwig and gives a glimpse too of what the young American student meant to Ludwig.

"Dear Professor,

"So you are happy in your new position, provided with everything necessary to wring from Nature her all too long guarded secrets. The world hopes,—she demands imperiously from you great results, which can only be accomplished with the severest labor. But the world will not thank you for them, rather she will look upon all that you do as only a duty, as a small return for what you owe. So it is when we are thrown entirely upon our own resources; quietly and alone the work goes forward and the nearer we approach to the eternal works of magnificent Nature, so much the happier we become; and finally we lose sight of all the petty intrigues around us and become wholly absorbed in our work.

"The traditional isolation of the life of the research worker is broken, when a spirit akin to his comes near. Those are the days of the greatest happiness when in the rivalry and the strife to understand Nature, now the one, and then the other, opens up his heart. So was it when we two worked together, but not often comes such fortune into one's life.

"You had so much of interest to tell me of your work on the stomach; gladly would I hear more of it, but perhaps it has not yet been printed. Send me a reprint when it appears, I hope in the near future. I rejoice over every step in advance: prodigal Nature is so niggardly with us research workers.

Faithfully yours, (Signed) CARL LUDWIG."

Mall had gone to Europe a crude and uncultivated, but very gifted youth; and in three years he had jumped to his full stature and returned a mature man far more European in type than American, except in his ardent love of democracy. His mind had in that brief time grasped the culture of Europe and from then on he seemed singularly adult; he became a directing force rather than the directed. More than anyone I have ever known he seemed released from fear of criticism from his fellows, because he was sure of the principles which guided him and because he saw the issues involved clearer than they. In this release from fear lay Mall's great strength.

The period of his return to America was one of remarkable awakening here. Gilman had already established a university along German lines in Baltimore and he and Welch were then planning a new medical school. Clark University was soon to be started, and then Chicago University—both founded as universities and not on the basis of the American college.

After the year in the laboratory of Prof. Ludwig, Mall wanted to take a year in pathology under von Recklinghausen, but it was now impossible for his father to help him further and he had to seek a position in which he might make his living. Mall had now decided to go into scientific medicine. In 1885 he wrote to his sister, "I can see back a year of great improvement in myself, I have made the acquaintance of and worked with two of the greatest professors living. Whether that will do me any good only the future can tell," and again, "My aim is to make scientific medicine a life work. If opportunities present I will. This has been my plan ever since I left America and not until late, since having received encouragement, have I expressed myself. I shall, no doubt, meet many stumbling blocks, but they are anticipated." The encouragement which he referred to was undoubtedly the faith of Ludwig in his ability. In 1885, William H. Welch, whom Mall had met in Leipzig, had been appointed Professor of Pathology in the Johns Hopkins University at Baltimore, as the first step toward the founding of the Johns Hopkins Hospital and Medical School, and Mall now applied to him for a position in the spring of 1886. Welch answered Mall

that the chances for scientific work in America were very scanty, but that he would try to get him an appointment. The appointment was, however, slow in coming and it was a period of great anxiety to Mall; of the event Dr. Welch has written to me: "When I started work at the Johns Hopkins in the autumn of 1885, I managed to have one of the twenty fellowships granted by the University given to pathology, and when Mall was ready to start he applied for one of these and became, I think, the first Fellow of Pathology. He helped to some extent with the teaching, but the fellowships were intended for research and he followed mainly his own lines. Among these was his study of the fermentative or digestive powers of the various species of bacteria upon the different tissues—fibrous, reticular, elastic—and he obtained interesting results, only in part published. His especial interest was reticulum. Mall was one of the group—Councilman, Halsted, Herter, Booker, Bolton, and others—who worked in the Pathological Laboratory before the Hospital was opened in 1889, a really creative, formative period for the future hospital and medical school."

Thus for the years 1886 to 1889 Mall was a Fellow, then Assistant, in pathology under Welch in the Johns Hopkins University. In the fall of 1886, Mall wrote to His that he planned to go on with the study of the intestine, and that his work would probably be from the pathological standpoint. But the events proved that he was wholly free to follow his own bent, which was more physiological. Mall seemed to jump at once to his full stature. His work on the stomach—just referred to in Ludwig's letter—representing perhaps his first work done without guidance, has always seemed to me one of his most interesting. Here was an organ, built from the same simple tube as the intestine embryologically, and yet entirely lacking the simplicity and the symmetry of the intestine. From every aspect it was divided into three zones which he studied first from their marked differences in thickness. By stripping the coats apart he found that every coat differed in the three regions, though the most striking difference was the thickness of the circular muscle coat of the pyloric region, fully twice as thick as in the middle region.

He observed that under varying functional states the dilatation of the pyloric region was slight; then by artificial distention, he found that at first each part expanded equally, until the folds of the mucosa were flattened out, after which the cardiac zone would continue to expand until it ruptured, while the rest of the organ showed little or no change. During digestion it was clear that some mechanism caused a regional distribution of blood, because the middle zone was always hyperaemic and there was an active contraction of its muscle coats, while the pyloric region remained firm, hard, and pale. Indeed, several hours after ligation of the coeliac axis, the middle portion would still be hyperaemic and its mucosa would be found completely digested, showing that its glands produced the digestive ferment, while the mucosa of pyloric and cardiac regions would still be normal. He passed in review all of the evidence then available for the view of Heidenhain that the chief cells secrete pepsin and the border cells the acid, since so convincingly demonstrated by Harvey and Bensley,[11] because the region containing the glands with these two types of cells, that is the middle, or fundic region was so favored by the circulation.

Unlike the intestine, which is supplied from one artery, the stomach is supplied from the coeliac axis, whose three branches, hepatic, gastric, and splenic, supply three different organs. Mall was concerned with finding out why the fundic region received the greatest blood supply. The anastomosing loops between the first branch of the superior mesenteric artery, with the hepatic, and then in turn with the gastric, he viewed as a continuation of the intestinal anastomosing loops, and from these arches the pyloric region was supplied with smaller, but at the same time, more numerous branches. The splenic system, on the other hand, with the right and left gastro-epiploic veins, formed a system of arches entirely peculiar to the stomach. Thus, while the pyloric part of the stomach was supplied from small hepatic branches, the fundic region, from which the pepsin and the acid were obtained, was supplied from both gastric and splenic vessels. This

[11] Harvey, B. C. H., and Bensley, R. R., *Biological Bulletin*, 1912, *23*, 225.

he said might prove to be of great significance because this is the zone with the greatest blood supply and because the spleen contains the most blood just at the end of digestion. The middle zone of the stomach he found supplied by the largest branches, which entered the submucosa and there formed a double network of arteries, the wider meshed one with free anastomoses. This wider meshed network lay in the middle of the submucosa, in contrast to the position of the major arterial plexus of the intestine which was closer to the circular muscle. The meaning of this striking difference in the position of the main distributing vessels he found by making an injection of the vessels of the stomach with the muscle cells still alive, and then forcing their contraction. This showed that the outer zone of the submucosa then became smaller in area, while the inner layers became folded with the mucosa, and the vessels were suspended between these two different zones of connective tissue fibers. The blood supply of the intestine had been adapted to bringing the capillary plexus beneath the functioning epithelium of the villi, that is, into the area of absorption. The pattern of capillaries in the stomach was quite different, for the arteries which penetrated to the mucosa broke at once into a candelabra-like arterial plexus around the depths of the gastric glands, this being the main functioning zone of the gastric mucosa, and there was a double plexus of veins, one just beneath the epithelium of the gastric pits on the surface of the stomach, and the other at the base of the glands. Thus, as Mall put it, the intestinal mucosa had two arteries, one to the crypts and one to the villus and one plexus of veins, while the gastric pattern was the reverse, one artery and two venous plexuses. In both cases, the arterial capillary plexus surrounded the main functioning zone of the organ, in the one case the surface, absorbing epithelium, in the other case the glandular, secreting epithelium. The transition between these two types of circulation he showed at the pyloric valve.

During these years Mall was still studying the intestine (20) with methods which might well be applied again. He became interested in the type of contractions and the influence which they had on the circulation. Measuring the cross section of the

different orders of vessels throughout their course from the mesenteric artery to the portal vein, he plotted a curve of these areas and showed that there were two areas of extreme expansion of the vascular bed: first, there was a sudden and marked expansion in the capillaries of the mucosa, followed by a fall in the veins at the base of the mucosa, and still more as they passed through the muscularis mucosae, and then succeeded by a still greater rise in the plexus of veins in the submucosa, leading finally to an area of the main portal vein not much larger than the original artery. In the plexus of the submucosa, where this remarkable expansion of vascular area occurs, are numerous retia mirabilia, still inadequately understood.

Mall studied the power of distention of the intestine on the basis of its connective tissue coats and the nature of its contractions on the basis of its muscular coats. The submucosa, isolated and macerated, or dried and distended, could be seen to be made up largely of white fibrous tissue, in the form of two opposing spirals of fibers, which came to lie more at right angles to each other as the wall was distended. He believed that this arrangement of the inelastic fibers was adapted to aid the muscles in changing the lumen of the intestine.

The elastic tissue, instead of being uniformly distributed between the white fibers of the submucosa, as it is in the skin, he found in two layers; one of these was very finely meshed and could be stripped off from the submucosal surface of the muscularis mucosae. It was filled with holes which marked the position of the mucosal arteries. The second elastic coat was of coarser mesh and lay outside the muscularis mucosae, and its mesh made a pattern for the bases of the crypts of Lieberkühn.

It was these studies that made Mall realize that the surgery of the intestine depended on the properties of the submucosa. Halsted told me that it was Mall who made this suggestion. In Welch's department in those early years, one sees an example of joint research at its best, for Halsted[12] worked out his methods of intestinal anastomosis, while Mall both assisted with

[12] Halsted, W. S., *American Journal of Medical Sciences*, 1887, *94*, 436, and *Johns Hopkins Hospital Bulletin*, 1921, *32*, 98.

the operations and studied the results from the structural standpoint. The histological work was published later by Mall (21) and showed that when the sutures entered but did not penetrate the submucosa, the best results were obtained. The submucosa, with its predominance of white fibrous tissue, was the only coat strong enough to hold the sutures; the muscle coat, of course, allowed the sutures to pull out, while when the needle penetrated the muscularis mucosae there were two dangers, one of infection along the sutures and, failing that, when the muscularis mucosae was torn, of a marked new growth of the glands into the zone of the submucosa.

In his study on intestinal contraction, Mall made wide use of the work of previous observers, showing how much he had learned under Ludwig and from his later reading. He saw that intestinal contraction had to be studied in connection with the blood supply, for on contraction the intestine invariably became paler, harder, and longer. Under Ludwig, Mall had learned of the two kinds of intestinal contraction, first, the local rhythmical, regular contractions, and second, the peristaltic waves. From his own studies, he found that there were three types of contractions to be considered, first, the rapid peristaltic waves so frequently seen after the death of an animal, which pass rapidly over the intestine and may go in either direction. This type of wave cannot be demonstrated in the living animal except under conditions that may be considered as pathological; and Mall considered they were not present during normal digestion and were only aroused by strong irritants.

The second form is the normal peristalsis of digestion, with a rate about 1/90 as fast as the abnormal contractions. This wave is always in the one direction, which Mall proved by getting Halsted to make several operations involving a reversal of an intestinal loop (22). The animals recovered from the operation and for a short time were well and then became ill. When killed it was found that the reversed loop was dilated most markedly at the proximal end, where there was a piling up of intestinal contents and an ulceration of the mucosa. This demonstrated

that the mechanism of the normal peristalsis lies within the wall of the intestine itself and that this wave is irreversible. The third, local, rhythmic contractions, now known by the work of Cannon with the newer technique involving X-rays and the fluoroscope to be significant in the breaking up of the food and the mixing of it with the digestive fluids, Mall studied with reference to the circulation. He found that each contraction of the circular muscle expelled blood from the plexus of veins in the submucosa and raised the pressure in the portal vein. He found that each of the rhythmic contractions in the intestine was followed by a wave in the superior mesenteric vein. Thus he studied the rhythmic contractions as giving rise to a venous pulse to aid in the hepatic circulation. He added these words (20, page 72): "Whether our rhythmic wave is present in the living animal has not yet been shown. This is a subject which, when investigated, will probably yield valuable and interesting results." And his final summary was (20, page 74): "To conclude, we may state that with this arrangement, each contraction of the muscle-walls of the intestine not only propels the contents of the intestine downward; not only aids in mixing the chyle, but also expels blood from the intestine into the portal vein, makes room for new blood, and thus acts indirectly upon the liver."

From the preceding pages, it is clear that the studies on the wall of the intestine made in Ludwig's laboratory, as well as the studies just described, focused Mall's attention on the subject of the fibers of the connective tissues. To this interest the work of His also contributed, based as it was on Bichat's concept of the importance of the connective tissues, an idea which His had analyzed and developed.

In the study of the intestinal and gastric walls, it had been the white fibrous coat (submucosa) and the elastic membranes which had occupied Mall's attention. Now he realized that the reticulum must also be considered. Years before Billroth (quoted by His in 1861) had called attention to the reticular framework of lymph nodes and had described it as made of multi-polar cells, but

both Ranvier [13] and Bizzozero [14] had shown that this framework is actually made up of a network of fibers on which the cells described by Billroth rest. In 1861 His [15] had shown that the reticulum is best seen when frozen sections of lymph glands are shaken in a test tube of water, until most of the lymphocytes are removed, and undoubtedly these preparations of His stimulated Mall's mind. Mall then discovered that the reticular network of fibers not only makes the framework of the lymphoid tissues, but also forms the supporting tissues of all the organs, and his work is considered as having established the fact that the reticular framework of organs is independent of cells. Mall showed that reticulum is so labile a framework that it adapts itself to and supports all of the functioning cells of each organ. Thus, when the cells of each organ, for example, the liver cells, the gastric glands, or all of the secretory cells of the pancreas are removed, the reticular framework so faithfully outlines the patterns of these cells that each organ can be told by the framework alone. When it is realized that this framework also conforms to the pattern of new growths of cells, the significance of this tissue, both in normal structure and in pathological processes can be grasped.

Mall discriminated the fibers of the connective tissues by various chemical means; on the one hand, the yellow elastic tissue, and on the other hand, the two closely related types, the white fibrous bundles and the reticular fibers. The yellow elastic tissue, with its protean forms, fibers, fine and coarse, dense networks and membranes, he found to be made up of two different substances, an outer, continuous membrane, not taking any stain, and an inner, discontinuous, stainable material, giving the appearance of fenestrae to the membranes. The elastic tissue he found resistant to acids and alkalies, unless boiled in strong concentration. It, however, could be digested slowly in pepsin, rapidly in pancreatin and papain. In this process, it was the inner

[13] Ranvier, L., Traité technique d'histologie. Paris, F. Savy, 1875, 689.
[14] Bizzozero, G., *Rendiconti, Real Ist. Lombardo,* 1872, *5,* 2.
[15] His, W., Untersuchungen über den Bau der Lymphdrüsen, Leipzig, W. Engelmann, 1861.

stainable material that was attacked by the digestive enzyme. The converse was true of the white fibrous tissue and the reticulum, for they were destroyed by acids and resisted digestion. These observations led Mall to the study of putrefaction, that is to say, he made use of bacteria as ferments and in these studies made interesting discoveries concerning bacteria. Thus, in a letter to Stanley Hall, April 21, 1889, concerning the position which Hall had offered to Mall at Clark University, Mall wrote:

"It is my plan to sail for Europe on June 1. I hope to pass what remains of the summer semester in Ludwig's laboratory. My acquaintance with the master ripened into friendship and I long to see him. At the same time, I desire to obtain some advice on certain points in physiological chemistry. My work this year, I am sorry to say, drifted into the subject of symbiosis and into the subject of putrefaction. Some of the observations are extremely interesting. A marked example is as follows. Certain anaerobic microorganisms will not grow when they are exposed to air unless they are mixed with facultative anaerobic organisms. I think also that I have considerably improved the method of cultivating anaerobic organisms."

He found that certain aerobic bacteria, which did not digest elastic tissue, nevertheless enhanced the power of anaerobes to digest it. Thus he discovered that the aerobic organism, in using up the oxygen, made favorable conditions for the growth of the anaerobic germs. These observations demonstrate Mall's originality in making important new observations in the then new field of bacteriology. He found that an anaerobic organism, which he got from garden soil and termed the "knob bacillus," on account of its tendency to grow spores at one end, when combined with Brieger's bacillus, gave a characteristic color reaction with elastic tissue, so constant as to be diagnostic of the digestion of elastic tissue. The effect of the bacteria on the elastic tissue he found to be exactly like that of the digestive ferments, namely, a preliminary splitting of the central stainable material of the fiber. He said that it was probable that the bacteria acted through ferments, though he could not isolate them. In the caseous tubercles in the lungs of cattle and of man, he found that the elastic tissue showed the same sort of damage as with the

digestive ferments, the process starting always in the center of the caseating material.

The property of the reticulated tissue in resisting the action of ferments made it possible to digest out the cells of the organs and reveal the reticulated framework. Mall's beautiful preparations of reticulum are known to all histologists. Of the lithographic plates of these preparations Ludwig wrote to Mall, November 16, 1890: "Day before yesterday on November 14th, I spread your beautiful plates before the astonished sight of the Fellows of our society."

Indeed, several letters from Ludwig showed that Mall had interested him so much in the experiments with digestion of connective tissue fibers that he was also working with the method. The plates referred to demonstrate how completely each organ can be identified from its framework. Some of the most beautiful of Mall's preparations were made with the spleen; the method was as follows: the spleen was removed, care being taken not to injure the capsule except for two small cuts at each end, into which small glass cannulae were tied. The entire organ was then submitted to digestion with pancreatin or with putrefactive bacteria. When the splenic cells were digested out, the spleen was attached to the water tap and washed with a slow stream of water until clear of debris. Then the entire reticular framework was immersed in a solution of magenta, and then distended and dried with air. The entire framework of reticulum was then exposed by dissecting off the capsule. In these original studies Mall found slight chemical differences between the reticular fibers and the white fibrous tissue, but eventually he believed that the major difference was morphological, in that the white fibers ran in wavy bundles and never branched, while the reticular fibers were always in networks. In this early work Mall said that elastic tissue is present only in vertebrates that have a bony skeleton and that it appears when the bones begin to ossify. Also, at that time, he believed that the fibers formed outside of the cells, which is the view accepted at present, though later, in a study of the development of connective tissue fibers, Mall changed to the view that the first fibrils

are formed within the cytoplasm of the so-called fibroblast. This view, on what is still conceded to be one of the most difficult phases of histology, is one of the few observations of Mall which has not stood the test of time. It is clear enough that to tell whether the delicate fibrils of the connective tissues are actually within or on cells is not possible by observation alone and the matter must be subjected to the experimental test of seeing whether fibers, chemically identifiable, can be developed without cells, as in tissue culture (Baitsell).

The presence of vasomotor nerves for the portal vein is regarded as one of the most important of Mall's discoveries. The first observation which led to this discovery was in the work on the intestine done in Ludwig's laboratory, where Mall (2) noted that the intestinal and mesenteric veins, interpolated as they are between two systems of capillaries, those of the intestine and those of the liver, had an unusually marked circular musculature and that under certain conditions of injection these veins were irregularly constricted. In 1890, Mall (8) published the first demonstration of motor nerves for the portal vein. In this paper he stated the problem clearly, saying that while in Ludwig's laboratory he had already seen constrictions of the portal vein due to contractions of the ring muscles of the portal vein; that when one considered that the portal vein was in lieu of a second artery to the capillaries of the liver, it was easy to hypothesize vasomotor nerves to this vein. To prove their presence it would be necessary first to eliminate all blood flow from the hepatic artery in the living animal and then stimulate the splanchnic nerves. Again in 1896, in his studies on the circulation of the stomach, Mall referred to the matter, saying (19, page 23):

"Recently I have found that irritation of the splanchnic nerve causes contraction of the walls of the mesenteric vein. All these influences brought together in all probability have a marked effect on the circulation through the liver and finally upon the circulation in general."

The final publications on this important subject, the first in German (13) in 1892, and the second in English (23) in 1896,

contained the complete proof of the action of those nerves. After Ernst Heinrich Weber showed that stimulation of the splanchnic nerves or irritation of the spinal cord caused a constriction of the arteries, it was thought that this was sufficient to account for the rise in pressure in the vena cava and aorta following the stimulation of these nerves. But Mall said that since the capacity of the arteries is insignificant as compared with the veins, it was necessary also to take into consideration the possible effect of a contraction of the veins, especially when it was shown that dilatation of the abdominal veins had a marked effect in lowering blood pressure. Mall proved the presence of motor nerves to the mesenteric and portal veins and their effect in raising blood pressure in the following manner. He found that when the aorta was tied just below the origin of the subclavian artery, the blood flow was completely stopped. This was long before the days of thoracic surgery and the operation, first made from the front at the level for good exposure of the splanchnic nerve was difficult and did not completely eliminate the flow of blood from the aorta. Mall finally devised an operation which was feasible, going into the thoracic cavity from the side, beneath the pectoralis major between the second and third ribs; from this position it was easy to tie the aorta just below the origin of the subclavian. With this method, the arteries below the ligature became entirely empty while the portal vein and its branches remained distended with blood. When in such a preparation the splanchnic nerve was stimulated, these veins contracted and emptied themselves completely. Then he showed also that even with the aorta intact, one could see the mesenteric veins and the portal contract under the stimulation of the splanchnic nerves, while an effect on the vena cava could not be determined on account of the pulsation of that vessel. He also demonstrated the rise in blood pressure in the portal. These studies carried the following acknowledgment to Ludwig (23, page 112):

"Most of the work recorded in this paper was performed in the laboratory of Professor Ludwig, and a portion of it dates from Clark University. It has been interrupted at various intervals and the reader will excuse its incompleteness. The diffi-

culty in performing the various experiments and the extensive bearing of the question are my apology. To exhaust this work will require many more hours of patient labor of combined forces (See also Thompson, *Arch. f. Physiol.,* 1893, and Bayliss and Starling, *J. of Physiol.,* 1894. Vol. 17). The substance of this paper has ben published in German (13) but in a different form; it bears the stamp of Ludwig. To that master I owe much,—all."

During the years that Mall was Fellow in Pathology under Welch, he worked on the development of the thymus. he made the model of the circulation of the vessels of the dog's stomach, he made all of the studies on intestinal contraction, the studies of intestinal anastomosis, and of the reversal of the intestinal loops with Halsted as well as the studies in bacteriology as applied to the fibers of the connective tissues and he performed the experiments proving vasomotor nerves for the portal veins just described. Thus were passed four active and remarkably fruitful years.

In the spring of 1889, Mall accepted a position as Adjunct Professor of Anatomy at Clark University, in Worcester, Massachusetts, and thus after years of training in embryology, physiology, bacteriology and pathology, he finally decided to go into anatomy, basing his decision on his interest in structure and on his talents as Ludwig had seen them. Stanley Hall, who had been in psychology at the Johns Hopkins University while Mall was in Welch's laboratory, was organizing the new university and Mall joined his faculty, which was as brilliant a group as was ever assembled in America. There was Michelson in physics, Nef in chemistry, Donaldson in neurology, Whitney in zoölogy, Mall, Lilly, Mead, Jordan, McMurrich, William Snow Miller, and others. It was to be a university in the true sense, with adequate time and facilities for research, and Mall spent three active years there. It was here that he finished the work on the reticulum and made the final experiments on the nerves of the portal vein. Most of his time, however, was now spent in embryology. Already while in Baltimore, or more especially in the summers at Woods Hole, Mall had been studying embryology, stating in his letters to His that he was working on the development of frog and fish embryos. Moreover, he began at this time the study

of the early development of the liver, seeking to reconstruct the liver lobule.

When Mall had returned to America, Professor His had given to him several human embryos cut in serial sections for study. When he had studied them he returned them to His who, however, gave them back to form the nucleus of a new collection to be made by Mall. Mall began to write short articles to be distributed to doctors about the preservation of the valuable human material that came into their possession. While still in Baltimore he had received a well preserved specimen, entirely normal, which he estimated to be twenty-six days old. It measured 7 mm. and had thirty-eight muscle plates. Mall had learned the Born method of three dimensional reconstruction with wax plates in His' laboratory and he now made several models of this specimen. It was closely like the well-known His embryo, Br_3. This was the first embryo ever modeled in America and at the time it was the most complete study yet made of any human embryo. This work laid the foundation for Mall's knowledge of embryology. Especially interesting to him were the form of the central nervous system and the cranial nerves. At this time he began the investigation of the coelom, making a cast of it by cutting out the plates and filling the cavity with plaster. Here again he showed the well developed form sense which Ludwig had noted. January 17, 1890, Mall wrote to His, from Clark University:

"My work progressed better than I had anticipated. The human embryo of which I told you (7.5 mm. long) has already been modeled after Born's method. The model brought out most decidedly the so-called neuromeres and many other points which were new to me. I am busily engaged with the liver and find that at first the liver cells grow into the omphalomesenteric vein and soon break it into many small vessels. I have also succeeded in isolating in large quantities the fibrils around the liver capillaries, as described by you. In section they seem to be identical with the reticulum of lymph glands. I hope ultimately to be able to give a reconstruction of the liver lobule and also its development."

And then later, December 19, 1900. "I have been quite busily engaged all autumn on the development of the liver and the pleuro-peritoneal cavity by means of corrosion methods. The way is quite round about. I first make the plate drawings and cut out the portions I desire to reconstruct, and then cast them with metal (Wood's metal). This method proves to be very valuable with arteries and all small spaces."

This was the start of Mall's interest in the development of the coelom and he ultimately went back to early stages, restudying the work of Budge on the development of the coelom in chick embryos. Budge had made injections of the true coelom and the extraembryonic coelom in chick embryos, and in later stages had injected the thoracic duct and thought that some of the original spaces of the coelom became the lymphatic system. It was these studies of Budge which suggested to Mall the problem of the origin of the lymphatic system, which he subsequently offered to me as a problem in his laboratory, and thus it is interesting to read (17) that Mall had studied Budge's actual specimens in His' laboratory. It proved subsequently that the lymphatics form much later, after the body cavities are well established. Mall followed with reconstructions all of the complex forms of the development of the pleural-peritoneal cavities, including the separation of them by the development of the diaphragm, as well as the forms of the greater and the lesser peritoneal cavities.

Mall now began to study the nervous system, specifically with reference to the development of the eye in amphibian forms, Amblystoma and Necturus. This work was begun at Clark University (15) and of it he wrote to His, January 31, 1892: "I hope that my communication will not be considered by you as worthless as you know I am not too well posted on the literature." As a matter of fact this paper is far more than just the embryology of an amphibian eye; it shows a remarkable insight into the problem of the histogenesis of the central nervous system, a subject then in its infancy. Mall's first conception of the neurone doctrine was based on the early work of Remak and

Helmholtz, who, soon after the discovery of the cell by Schleiden and Schwann, had postulated that the nerve fiber is an outgrowth of a nerve cell. This concept was then made more likely through the discrimination of axone and dendrite by Deiters as well as by the now well-known methods for specific staining of neurones devised by Golgi and Ramon y Cajal. His had, however, brought the strongest evidence of the outgrowth theory by direct observation in embryos of a nerve fiber which was a continuation of a single nerve cell. This concept was subsequently conclusively established in Mall's department by Ross G. Harrison[16] many years later, in 1910, by watching the outgrowth of the fiber in tissue cultures, a method which he devised for the specific study of this problem. In 1893, Mall judging that the balance of evidence at that time was in favor of the neurone doctrine, developed the concept that there was a specific polarity of the developing neurone, in that the receiving pole of the cell always pointed to the surface of the ectoderm, or toward the central canal of the central nervous system. The lining of the central canal was, of course, originally surface epithelium on account of the involution of ectoderm to make the nervous system. Mall described the pattern of each of the sense organs, from the simplest one in the olfactory nerves to the most complex in the retina, on the basis of this polarity. He observed that not only in the retina but in the central nervous system as well the polarity of the nerve cell was foreshadowed by a constant position of the axis of cell division, the axis always being parallel to, or the plane of the spindle perpendicular to the original ectodermal surface. The concept involved specific growing zones in the nervous system, which in the developing retina he found to be always in the periphery of the optic cup and primarily at the surface of the central canal. W. Müller[17] had suggested in 1874 that the optic nerve arose in the retina and grew to the brain; this was demon-

[16] Harrison, R. G., *Journal Experimental Zoölogy,* 1910, 9, 787.

[17] Müller, W., Festgabe an Carl Ludwig, *Beiträge zur Anatomie und Physiologie,* Leipzig, F. C. W. Vogel, 1874.

strated later by His [18] and by Martin in 1890 [19] for mammalian material, and in the next year by Froriep [20] who reported a shark embryo in which the optic nerve had started from the retina but had not yet reached the brain. Mall now reported the same observation in the amphibian material. This series of reports in quick succession on this important point shows that it was a timely subject. On January 31, 1892, Mall wrote to His about his observatons on the retina and the growth of the optic nerve in amphibia and enclosed a diagram in which he outlined his concept of the polarity of the neurone, as a general law of growth of the nervous system. To this letter His replied, February 27, 1892, "Your scheme for the growth of fibers of the neuroblasts has, however, exceptions. In the forebrain, most of the cells develop their fibers toward the ventricle." To this letter Mall replied, March 17, 1892:

"Your objection to the 'polarity of the nerve cells' I think can be met. Recently the beautiful paper of von Lenhossék, as well as my observation that various sense organs in Necturus develop fibers to the central nervous system, gives new evidence. The ear, according to Ayers, is formed the same way. As regards the cerebrum I think that the sketches I enclose will also overcome the difficulty. The case you mention to me is identical with the young retina where also the cells are directed in the wrong way. This may only indicate that cells are wandering.

"In order to form the grey substance in the brain or the commissures, the cells would have to develop their fibers on the wrong side. . . . But the more I think of it, the more I believe that there is a great law of growth at the bottom of it all and that the polarity of cells is present in all animals. I hope that you will

[18] His, W., Histogenese und Zusammenhang der Nervenelemente. *Archiv für Anatomie und Entwickelungsgeschichte,* 1890, Supp.-Bd., 95-117.

[19] Martin, P., Die Neuroblasten des Oculomotorius und Trochlearis. *Anatomische Anzeiger,* 1890, *5,* 530-532. Zur Entwickelung der Netzhaut bei der Katze. *Zeitschrift für vergleichende Augenheilkunde,* 1893, *7,* 25-41.

[20] Froriep, A., Ueber die Entwickelung der Seenerven. *Anatomischer Anzeiger,* 1891, *6,* 155-161.

forgive me for speaking of theories instead of facts, but I believe they will cover all the known facts with the exception of the spinal ganglia." In the paper on the retina, Mall explained that the difficulties in following the cells of the spinal ganglia were due both to the fact that in their division after separation from the neural crest all orientation to the original position was lost and also that the sensory cells, originally bipolar, soon became unipolar. Continuing the letter to His, he said, "At any rate, if you will be forgiving and feel that the question is worth criticizing, I shall be very thankful." Then on December 27, 1892, Mall wrote again to His, now from the University of Chicago: "I am at present occupied with the polarity of cells in the various portions of the brain, and find that it holds in many portions, as in the corpus dentatum and olivary body." Again, Oct. 9, 1893, he wrote to His, "I have sent you a small paper on the retina. Forgive it." These references to the patterns of growth of neurones in the letters to His are exceedingly interesting in the light of the theory of neurobiotaxis as developed by Ariëns Kappers. Realizing that he was dealing with an obscure field with somewhat vague hypotheses, Mall clearly foreshadowed Kappers' theory that throughout the central nervous system each group of developing neurones is oriented in the lines of the incoming sensory impulses. Thus again one sees the originality of Mall's mind, for, in taking up a new field, neurology, he made observations which have stood the test of time, sensed their meaning and judged the force which directs the path of the growth of neurones.

When Clark University was first founded, it had seemed to its faculty that it might develop into what we now know as the research institute; but two factors militated against this, first that Jonas G. Clark, who founded it, was not sympathetic toward this idea nor had he any realization of the endowment necessary, and secondly that Stanley Hall failed to take his faculty into his confidence over his difficulties and that he lacked the power to create for them the peace of mind essential for research. It is now known that when Mall once realized that the others felt as he did, he took the steps which solved the difficulty. Mall

was an intimate friend of R. F. Harper, the brother of President Harper, then organizing a faculty for Chicago University and in the spring of 1892, Whitman, Michelson, Nef, Bolza and Mall went to Chicago University.

The correspondence between Mall and Harper shows that it was Mall who persuaded Whitman to go to Chicago University, a decision so momentous for Chicago. This, however, was not his only service to the University, for though he remained there only one year, through his friendship with President Harper and through the wisdom of his counsels, Mall not only founded the Department of Anatomy in the University but he was a powerful factor in the organization of the whole biological department. Here he developed the plans by which each department of the University might develop into an Institute, with the encouragement of research as its major activity both for faculty and for students.

In the spring of the next year, 1893, Mall was offered the Professorship of Anatomy at the Johns Hopkins University, where the new medical school was to be opened in the fall. This was the third professorship which Mall was offered in four years' time—Clark University, Chicago University, and the Johns Hopkins University. It was, of course, Welch who had called him back to Baltimore and Mall made the decision on the score that he could more quickly organize a department there to his liking, one in which he could devote a greater proportion of his time to research. He was now thirty-one years old and with this decision the period of his training may be considered as complete. His work during the years between twenty and thirty had shown his originality and his power in mastering new fields. He had established the endodermal origin of the thymus and clarified its relation to the lateral line sense organs; he had worked out the vascular supply of intestine and stomach so completely that the work still remains the standard; he had studied the physiology of the movements of the intestine and had aided in working out the surgery of intestinal anastomoses; he had discovered the vasomotor nerves of the portal vein; he had improved the methods for cultivating anaerobic bacteria, showing that they would grow

in the presence of facultative anaerobes because they used up the oxygen; he had used bacterial ferments as well as digestive ferments to work out the nature of the reticular framework of organs and had made valuable contributions to an understanding of the laws underlying the growth of the neurones of the central nervous system.

Mall's mature years were spent in Baltimore; his profound influence on medical education, his share in the founding of anatomical journals, in the leadership of scientific societies, and finally in the establishing of a Research Institute for Embryology are part of the story of his life. His effect on medical teaching in this country was due not only to the originality of his ideas on education but also to the power of his example; gifted with powers of organization, he nevertheless continued to put his major efforts into research to the very end.

His research work in Baltimore may be classified under three heads: embryology, the structure of organs in the adult as adapted to their functions, and a beginning in anthropology.

Mall now made a series of embryological studies, all closely connected in thought; they included the development of the diaphragm in human embryos, the development of the ventral abdominal walls, the development of the body cavities and of the loops of the intestine. In the study of the diaphragm and of the body walls he was interested in the primary relation of the nerve to its myotome or muscle mass as a guide to the amount of wandering and differentiation of muscle groups. Subsequently he suggested the study of this principle to two of his students, Dr. Charles R. Bardeen and Dr. Warren H. Lewis, who followed the development of the muscles and nerves of the arm buds in human foetal stages. Concerning this work, His wrote to Mall that the pages of the *American Journal of Anatomy* gave brilliant evidence of Mall's activities and those of his students. The figures of the body wall and of the extremities he thought excellent and that they showed a rich material.

From the study of the early stages of the development of the liver, Mall was led to the consideration of the whole development of the body cavities and of the loops of the intestine. Both

of these he followed by means of three dimensional, wax models, carrying the subject through to the condition of the adult. In connection with the coelom, Mall started with the very early stages, when the extra-embryonal coelom is just being incorporated into the body to make the coelom and His wrote to him that he was especially glad that he had made clear the early forms of these cavities. For the earliest stages of the intestine he depended on the embryo of Graf Spee, in which the endoderm had not yet been incorporated into the body of the embryo. In Mall's youngest stage there was already a fore gut, a mid gut and a hind gut, and from then on the intestine showed the same shift in position in development as the muscles, in this case, however, determined by the position of the arteries to the umbilical vesicle which showed a shift of at least ten segments. Mall determined certain fixed points of reference. While this shift was taking place, the intestinal loop bent so that the oral end was bent toward the right and the aboral toward the left; the right loop then was the forerunner of the small intestine and the left the large gut. Thus was formed a primary loop of the intestine and already in this primary loop Mall could mark out the definitive loops of the small intestine, as indicated by the branches of the superior mesenteric artery. These loops he found more constant in their position in the adult than the convolutions of the brain. Of this work His wrote that it was the very first time that the development of any organ had been carried from its early stages through the transition forms to its condition in the adult and that he regarded this as a great advance.

There was a slaughter house near the anatomical department in Baltimore, from which an abundant supply of embryo pigs in every stage could be obtained in fresh state, with the heart still beating. With this material Mall started the study of the development of the blood vessels, a problem which he gave to a series of workers—J. B. MacCallum, H. M. Evans, G. L. Streeter, and myself. Likewise with the same material Mall suggested to me the problem of the origin of the lymphatic system while the growth of the lymphatics in amphibian forms was fol-

lowed by E. R. and E. L. Clark. These problems became thoroughly identified with Mall's laboratory. The study of the development of the blood vessels led to the fundamental concept that blood vessels come from cells, that is angioblasts, and that endothelium is the primary tissue of the vascular system; the progression of the development of the vascular system was shown by Dr. Streeter to be determined by the functional or developmental needs of each part at each stage in growth rather than as a foreshadowing of the pattern of the vessels in the adult. The work on the lymphatic system likewise led to a concept of the fundamental nature of the endothelial cell; the theory involved the idea that lymphatics are modified veins and have the same relation to the tissue spaces that the blood vessels have. In the development of these concepts Mall took the deepest interest.

There are a number of general problems in embryology of great practical significance to medicine and to human welfare which depend upon large collections of embryos and embryological institutes. These problems attracted Mall even when he first started his collection; and, with the full development of his powers, they became increasingly significant to him, until at the time of his death he was almost wholly occupied with them. The study of his publications on these subjects as they followed year after year enables one to trace both the growth of the subject and the development of Mall's power and critical judgment. His "Plea for the Foundation of an Institute of Embryology" had as its climax a clear statement of the nature and the significance of these problems.

Of first importance was the question of the age of embryos, both with relation to the time of conception and to the more general problems of growth. The history of the efforts to determine the duration of pregnancy and the age of embryos covers centuries and was outlined by Mall in 1910 in his chapter on the Age of Embryos and Foetuses in the Human Embryology, edited by Keibel and Mall. Mall's chapter was written seven years before the discovery of a method of following the oestrous cycle

in guinea pigs by Stockard and Papanicolaou [21] which opened up the comparative aspects of this subject for experimentation. It was likewise before the discoveries of the hormones which control the reproductive cycle and all of the resulting experimental work on this cycle, such as that of Leo Loeb, Edgar Allen, H. M. Evans, G. W. Corner, and C. G. Hartman. Lacking the data from the comparative side which indicate that the oestrus and ovulation in animals are synchronous but that menstruation probably occurs in the interval between ovulations, Mall concluded from the statistical analysis of human material that the relation between ovulation and menstruation was inconstant, coinciding in about two-thirds of the cases, and that fertilization tended to occur just before or just after menstruation. As soon as it was determined that fertilization of the ovum takes place in the tube or around the ovary and not in the uterus, it was clear that a certain variation in the relation of fertilization to menstruation occurred. Lacking reliable data as to time of occurrence of ovulation and its relation to the menstrual period, Mall was unable to harmonize the variations that occurred in his assembled records. He recognized this and pointed out that the real age of the embryo is the ovulation (or fertilization) age and not the menstrual age.

Mall analyzed all of the known data on measuring embryos. The difficulties are associated with the fact that the earliest embryos are straight and then curved, and finally straight again, so that the greatest length cannot be correlated with growth. On this account it was essential to determine certain fixed points for measurement and Mall judged that of all the measurements the crown-rump and neck-rump are the best. The curvature being at the anterior end of the embryo, making the rump point fixed, the two most constant anterior points are the center of the midbrain and the line between the skull and the cord, that is, the foramen magnum. He showed that the latter can always be determined by the extension of a line which connects the center of the

[21] Stockard, C. R., and Papanicolaou, G. N., The existence of a typical oestrous cycle in the guinea pig with a study of its histological and physiological changes. *American Journal of Anatomy*, 1917, *22*, 225.

eye with the center of the external auditory meatus. Added to the difficulties associated with the curvature of the embryo itself, is the fact that few of the embryologists making the measurements have been trained in methods of measurement developed by the anthropologists. Mall was fully aware of the latter difficulty, and making his measurements with proper instruments and with the care of an expert, he constructed a mathematic growth formula, represented as a curve, which makes him, according to Scammon, one of the pioneers in biometrics.

At the beginning of his study of the pathology of embryos, Mall had even less aid from preceding work than for the determination of the age of embryos, in spite of the extensive work on teratology. Three studies, a report of forty-five aborted ova in 1834 by Granville,[22] the work of Giacomini,[23] and of His[24] gave material which he used as freely as his own. Mall gave four reports of his material; the first (40) was published in 1900 in the volume of the Johns Hopkins Reports in honor of Dr. William H. Welch, in which he reported fifty-three pathological ova; three years later he described twenty more (48); in 1908, in his study of the causes of human monsters (71), he reported 163 specimens from the first 400 specimens of his collection, and in the final report, published after Mall's death, as a joint work of Mall and A. W. Meyer (104), all of the pathological ova, namely, 353, in the first 1,000 specimens of the Mall collection, are analyzed.

The most fundamental question in relation to pathological embryos is that of heredity versus environment. The whole subject of genetics has grown up since Mall started his work and has brought a flood of light from the comparative standpoint to this phase of the work. That the formation of pathological embryos, anomalies and monsters are due to environment as well

[22] Granville, A. B., Graphic Illustrations of Abortion, [etc.]. London, J. Churchill, 1834.

[23] Giacomini, C., *Ergebnisse der Anatomie,* 1894, *4,* 617-649.

[24] His, W., Anatomie menschlicher Embryonen. 2 vol., Leipzig. F. C. W. Vogel, 1882, and Festschrift, Rudolph Virchow, gewidmet zur Vollendung seines 70. Lebensjahres. *Internationale Beiträge zur wissenschaftlichen Medicin.* Berlin, August Hirschwald, 1891, *1,* 177-193.

as to heredity is one of the discoveries of modern medicine of far-reaching value to human welfare. Mall credited this important discovery to the experimental embryologists and estimated that the work started in 1888 with the observations of F. Vejdovsky [25] who noted that the eggs of Lumbricus produce more monsters in warm than in cool weather and thought that this was due to the change in temperature. Then followed the work of Driesch [26] who submitted sea urchins' eggs to high temperatures in the two cell stage and obtained double monsters; and of O. Hertwig [27] who produced spina bifida experimentally; and finally the work of Stockard.[28] In his paper on Monsters (71), in the report on Cyclopia (98), and in Chapter VII of the final paper (104), Mall analyzed the history of experimental embryology, giving the results of submitting eggs to different chemical environments, of treating them with X-rays, or of operative procedures, giving most credit with respect to cyclopia to the experiments of Stockard who found that by treating fish eggs with substances of the nature of anaesthetics at certain critical stages, he could produce anomalies at will. For example, by adding magnesium chloride to the sea water in which were fish eggs at the time of marked cell division which precedes the formation of the optic cup, fifty per cent of the eggs developed into cyclopia. Of these experiments, Mall said (See 98, page 9): "The remarkable experiments of Stockard set at rest all germinal theories of cyclopia and prove that every egg has in it the power to develop cyclopian monsters." With wider application, it is clear that an abnormal environment, when the anlage of different organs are in a critical state, accounts for other abnormalities. Mall's final conclusions (104, page 200) were that the study of pathological embryos and the recent experiments in embryology set at rest for all time the question of the causation of monsters. He said, "It has been my aim to demonstrate that the embryos

[25] Vejdovsky, F., Entwickelungsgeschichliche Untersuchungen, Prag, 1888.

[26] Driesch, H., *Zeitschrift für wissenschaftliche Zoölogie*, 1892, *55*, 1-62.

[27] Hertwig, O., *Archiv für mikroscopische Anatomie*, 1892, *39*, 353.

[28] Stockard, C. R., *Archiv Entwcklungsmechn. Organ.*, 1907, *27*, 249.

found in pathological human ova and those obtained experimentally in animals are not merely analogous or similar, but identical. A double-monster fish or a cyclopian fish is identical with the same condition in human beings. Monsters are produced by external influences which act upon the ovum, as, for instance, varnishing the shell of a hen's egg or changing its temperature, traumatic and mechanical agencies, magnetic and electrical influences, as well as alteration of the character of the surrounding gases, or the injection of poisons into the white of an egg. In aquatic animals monsters may be produced by similar methods."

In the study of his own material, it was necessary to formulate some basis for classification, which Mall regarded as only tentative because it had to be based on the nature of the pathological process rather than on etiology.

In developing fertilized ovum, the materials that are to form the mechanism of implantation and the enveloping membranes are segregated early. During the process of cleavage, these trophoblastic elements take their place as a surface layer of cells which undergo rapid division and precocious differentiation. Soon after arrival in the uterus, the trophoblastic elements can be recognized morphologically and as they begin to function, the ovum becomes converted into a blastocyst, the greater part of whose wall is of trophoblastic origin, only the inner cell mass giving rise to the embryo proper. Thus Mall was able to classify pathological ova in accordance with the degree of their attained development. He recognized seven groups: the first two had trophoblast alone; while the other five had both trophoblast and embryo. The first group comprised those having only a partially developed trophoblast while the second consisted of a complete chorion and an exocoelomic cavity. The rest were classified by the size of the embryo into those in which it was a mere nodular rudiment, or with embryos in progressive states of completeness.

The conditions which produce pathological ova, in so far as they originate in the environment act through the trophoblast. Before implantation the nutrition of the embryo passes through the chorion into the fluid of the exocoelom; after implantation, the interchange of materials is through the blood vessels. Mall

summed up the process by which an abnormal environment can affect the embryo under the term *faulty implantation.* When the abnormal environment was brought to bear on the very early stages, there resulted the complete disappearance of the embryo, as in the first two groups in his classification. Likewise the other stages corresponded to conditions in which the abnormal factors were applied later. Dr. Mall probably went too far in his stressing of the environment as the cause of all pathologic ova. Had he lived ten years longer, he would certainly have modified this emphasis. At least it was a demonstrable cause, and so much better than prenatal impressions which it supplanted, that one does not wonder at Mall's enthusiasm.

The most imporatant work of Mall in connection with the subject of pathological embryos was his analysis of the nature of the changes in the embryo itself. He found that these embryos did not show the same type of lesions to be found under abnormal conditions in the adult. Two factors bring about marked differences; first, the early embryos have no leucocytes, for the first blood cells are all red blood cells. This at once eliminates all of the processes of inflammation of the adult which are associated with the migration of leucocytes, both their presence in the tissues and the effects of their enzymes. This means that the reactions in the connective tissues are those of the primitive mesenchyme cells, fibroblasts, and macrophages. Secondly, during development, there are hormones which control and regulate the relative growth of the different tissues and organs. Mall found that the most striking effect of pathological conditions was an interference with this regulation. In his earliest descriptions of the phenomenon, he referred to the effects as a "dissociation" of the tissues. In his final report, he recognized that this phenomenon received its explanation in the experiments of Harrison on tissue culture. In tissue culture, cells grow outside the body, wholly released from the regulating mechanisms which integrate growth in normal development; and Mall saw that the phenomenon which he had called "dissociation" in pathological embryos was a growth of cells in the embryo exactly like that of tissue cultures. Besides these fundamental principles of the pathology of

embryos, Mall's reports give the most extensive accounts of pathological embryos in existence. The nature of the abnormalities in the environment was the point on which he was working at the time of his death, and the importance of this study is indicated in the fact that fully 20 per cent of all pregnancies end in abortion (77, Vol. I, page 203).

It is probable that the best known work of Mall's mature years is that on the spleen, the liver, and the heart. All of these studies represent years of work and they call to mind the words of Councilman [29] about Mall: "Mall was a great scientific investigator. As such his work was thorough; he touched no subject on which his investigations did not throw light and in most cases he left the subject standing clearly, the obscurities gone."

The work on the spleen began with his study of the reticular framework already referred to and the introduction to his second paper on this subject published in 1900, with the interesting title "The Architecture and Blood-Vessels of the Dog's Spleen," shows the direct continuation of his thought, for he said, "In reworking the entire trabecular and vascular systems of the spleen, I have employed all methods at my disposal in order to obtain a clear picture of the whole organ. Throughout the work my aim has been to study the coarser structures first, then in order the finer and finer, hoping in this way to find some histological unit which repeats itself a great many times to produce the whole organ. That the use of a variety of methods is required to unravel this difficult organ in a satisfactory manner is clear to all who have studied this subject. It may not be out of place to state that I have worked a considerable time upon the spleen each year during the last fourteen years." The most outstanding discovery in this paper is that the veins of the splenic pulp have an incomplete endothelial lining, a discovery which is often credited to Mollier some years later. This establishes the fact of an open circulation in the spleen. Mall described the histological unit of the spleen with the artery and its lymphoid follicle in the center and the trabeculae with the interlobular veins and their remark-

[29] Councilman, W. T., Franklin Paine Mall (1862-1917), *Proceedings American Academy of Arts and Sciences*, 1922, 57, 495-499.

able longitudinal muscles on the periphery. These units were about one millimeter in diameter and the average number for the dog's spleen was 80,000. In this paper and then later in 1903, Mall was interested in what has long been the major problem in connection with the spleen, namely, the nature of its circulation. The second paper was published shortly after Weidenreich's, with whose conclusions Mall was in agreement. Mall devised many types of injections, arterial and venous, especially methods for filling the pulp spaces. For many years the main question at issue was whether there were special channels between artery and veins of the pulp distinct from the route from artery through the pulp spaces into the veins. By means of injections with asphalt, Mall showed that the route from artery to vein was through the pulp spaces. Thus the circulation of the spleen is an open one; the walls of the artery are open except where they pass through the Malpighian follicles, and from the openings blood passes into the pulp spaces. Mall made what he regarded as the crucial experiment for demonstrating the circulation through the spleen; he tied the splenic veins of the hilus in two dogs under anaesthesia, and then returned the spleen to the body cavity for a half hour. At the end of this time, when the spleen showed maximum distention, he tied the arteries in one animal, cut out the spleen, and fixed the organ in formalin for twenty-four hours with the capsule intact and then in frozen sections found the pulp spaces engorged with blood. In the other animal he cut the veins, watched the contraction of the organ, which took a few seconds, and produced a spleen entirely free of corpuscles. He also obtained the spleen in its psysiological state by paralyzing its muscle by injecting nitrites into the arteries. His work on the framework showed how the spleen was constructed in order to bring about the emptying of the pulp spaces into the veins of the pulp, for when the longitudinal muscles in the veins of the trabeculae contract, the entire lobule becomes smaller and its veins compressed, while the veins within the trabeculae open up to receive the blood flow. Thus the spleen is an organ in which the blood readily flows into the tissue spaces, the so-called pulp; but unlike other organs it has an efficient

mechanism, the smooth muscle of the trabeculae, for speedily bringing the blood back into the circulation. These concepts of the open circulation of the spleen have been fully confirmed by the physiological work, especially of Barcroft (1926), in recent years.

Mall's interest in the architecture of the heart began with his reading of the paper of Gerdy [30] on the architecture of the heart. When, in 1891, Krehl [31] of Leipzig, showed that the auriculoventricular rings were really the tendons for the bands of the heart muscle, Mall saw that with this foundation another step forward could be taken and suggested to one of his most brilliant students, John Bruce MacCallum, that he try to unravel the heart muscle bands. This MacCallum did in the brief period of three weeks, using the embryo pig, and the material showed that the main bands started in the right atrioventricular ring, made a vortex at the apex, then passed through the septum from front to back and ended through the chordae tendineae in the auriculoventricular ring of the opposite side. Those which started on the front of the heart ended on the back of the opposite side. By cutting one small bundle, MacCallum found that he could unroll the foetal pig's heart into a single sheet or scroll of fibres.

Some years later, after MacCallum's death, Mall carried this work to completion by unrolling the adult human heart. He showed that there are two primary spiral bundles, which he termed sino- and bulbo-spiral, which make the vortices at the apex of the heart and empty the heart by a contraction which one might term spiral, but in simpler words, is like the wringing out of a cloth. Later he studied the development of these primary bundles and the origin of the atrioventricular bundle, but he recognized that our knowledge of both of these subjects is far from complete.

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[30] Gerdy, P. N., Recherches, discussions et propositions, [etc.] *Thèse*, Paris, 1823.

[31] Krehl, L., Abhandlungen der mathematisch-physischen Klasse der königlichen sächsischen Gesellschaft der Wissenschaften, Leipzig, 1891, *17*, 341-362.

Mall's paper on the structure of the liver shows his powers at their full. This paper represents the work of years; he said that when he started with the study of this organ he had thought that its unit would be the easiest to determine, but he had found it the most difficult. Starting with the work of His on the origin of the liver in its earliest stages, Mall followed through the complete development of the vascular channels through their complex changes, with the venous blood coming first through the omphalomesenteric vessels and then through the umbilical vein, and finally through the portal vein. The introduction to this paper contains an analysis of Thoma's hypothesis on the laws of growth of blood vessels which leads up to the principle that the capillary is the primary unit of the vascular system. In Mall's words (58, page 252): "The anlage, then, of the vascular system is the capillary; artery and vein are secondary and are differentiated out of them by the flow of blood set in motion by the heart." This is one of the fundamental generalizations that came from Mall's laboratory, that throughout the vascular system, including the lymphatics, endothelium is the essential tissue, muscle coats and connective tissue coats are accessory, or as their name suggests adventitial.

The question Mall attempted to solve in the liver was the nature of its structural unit; by means of corrosions of hepatic and portal veins, he could reconstruct the pattern of this organ and determine that the so-called lobule, with its center at the hepatic vein, can not be considered a structural unit, since the lobules vary so greatly in size. On the other hand, the portal units, as had been noted years before, in 1888, by Sabourin[32] met all the conditions of a structural unit, since all of them are of the same size. The portal units also have the artery and bile ducts in the center, as well as the portal vein, and have a size that is determined by the length of the capillary bed of the organ. To obtain a picture of the portal units of the liver in three dimensions is of such difficulty that it probably cannot be gained from pictures alone, but the study of the corrosions of the vessels which

[32] Sabourin, C., Recherches sur l'anatomie normale et pathologique de la glande billiare de l'homme, Paris, F. Alcan, 1888.

Mall left in his laboratory makes this possible. Mall showed that this concept was essential for following the development of the liver; it also clarifies all the pathological changes involving congestion. In this study of the liver Mall brought to its fullest fruition the concept of structural units, which he first formulated in the study of the villus in Ludwig's laboratory. The concept is that there is a structural unit for each organ which is a unit of function.

These pages have not exhausted the story of Mall's scientific work but they have touched upon his most important studies. In summary, he established the endodermal origin of the thymus; demonstrated the vascular patterns of organs; discovered the vasomotor nerves of the portal vein; clarified the structure of organs by his concept of structural units; threw light on the laws of growth of the nervous system; followed the development of certain organs to the adult state; laid the foundations for the study of the pathology of embryos, discovering why these abnormal processes differ from those of the adult. He was an investigator, an educator and a leader; and when the history of medical education in this country is written, it will appear that his thought was a significant contribution to its advance.

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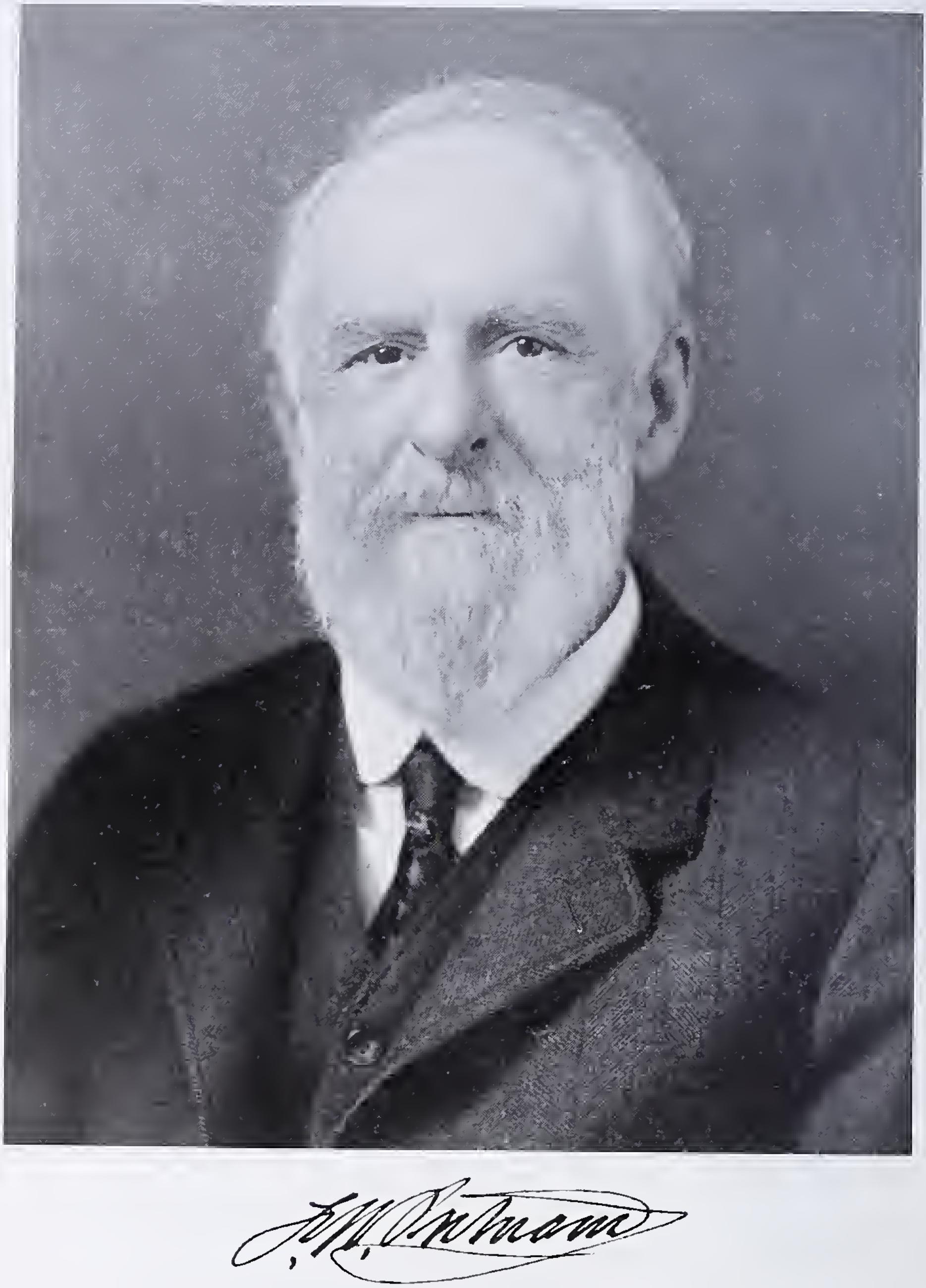

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XVI—FOURTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

FREDERIC WARD PUTNAM
1839-1915

BY

ALFRED M. TOZZER

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1933

FREDERIC WARD PUTNAM

1839-1915

BY ALFRED M. TOZZER

To have been, almost literally, the founder of a new branch of knowledge in America is, in itself, no small honor, and when to this is added the prime force behind anthropology for almost fifty years, the record is indeed unusual.

From an ancestry dating back to John Putnam who migrated from Aston Abbotts, Bucks, England in 1640, including Appletons, Fiskes, Wards, and Higginsons, Frederic Ward Putnam was born in Salem, April 16, 1839, and died in Cambridge, August 14, 1915. He was the son of Eben and Elizabeth (Appleton) Putnam, grandson of Eben and Elizabeth (daughter of General John Fiske) Putnam and of Nathaniel and Elizabeth (daughter of Joshua Ward) Appleton. In 1864 he married Adelaide Martha Edmands, who died in 1879. Three children were born, Eben, Alice Edmands, and Ethel Appleton Fiske, later married to John Hart Lewis; she died in 1922. In 1882 he married Esther Orne Clarke who survived him until 1922.

During his whole career Putnam was a natural historian in the old-fashioned, but best sense of the word. With almost no school or college background, but with home tuition, he developed at a very early age a great interest in nature. He was an assistant to his father in the cultivation of plants, but it was the study of birds which especially interested him. His first two scientific papers, on the fish of Salem Harbor, were published at the age of sixteen. With little help, and mainly on his own initiative, the next year he published a list of the fish and a catalogue of the birds of Essex County. These unusual achievements brought him almost immediate recognition, as he soon became Curator of Ornithology in the Essex Institute of Salem. This was his first official connection with a scientific institution, but this relationship, which later came to include institutions from California eastward, continued for sixty years until his death.

Within a year, Professor Louis Agassiz discovered him at Salem and induced him to become one of the assistants in the Museum at Cambridge. While still retaining his position in Salem, he worked with Agassiz for seven years. With him at

Cambridge were Edward S. Morse, Alpheus Hyatt, Samuel H. Scudder and A. S. Packard.

During this time he turned from ornithology to ichthyology. While his contemporaries were going through the usual college courses, he was studying living forms and making valuable contributions to science. The discipline of routine education he never had; his discipline came from nature and his associations with his one teacher and his fellow students, all of whom became noted scholars. For his work with Agassiz, Harvard conferred upon him the degree of B. S. in 1862. The influence of Agassiz remained with him during his entire life. His love for his teacher and the respect and admiration for Agassiz's method of teaching were always favorite themes in his conversations with his own pupils. His never-ending lament was that his students found their knowledge in books rather than in specimens. His work in ornithology and ichthyology is shown in the following list of positions he held from 1856 to 1889:

1856-1861—Curator of Ornithology, Essex Institute, Salem.
1862 —Curator of Ornithology and Mammalogy, Essex Institute.
1863 —Curator of Mammalogy and Ichthyology, Essex Institute.
1864-1867—Curator of Vertebrates, Essex Institute, Salem.
1864-1870—Superintendent and Director, Essex Institute.
1859-1868—Curator of Ichthyology, Boston Society of Natural History.
1867-1869—Superintendent, Museum East Indian Marine Society, Salem.
1869-1873—Director, Museum of Peabody Academy of Sciences.
1876-1878—Assistant, Ichthyology, Museum of Comparative Zoology, Cambridge.
1882-1889—Massachusetts State Commissioner of Fish and Game.
1874 —Assistant, Kentucky Geological Survey.

1876-1879—Assistant to United States Engineers in the Surveys West of the One Hundredth Meridian.

His interest in anthropology may be said to have begun as early as 1857 while attending a meeting of the American Association for the Advancement of Science in Toronto. On one of his frequent excursions into the country in search of information he found on the site of Mount Royal a quantity of clam shells, fish bones, burned earth and pottery. This was one of the first reported discoveries of the work of ancient man in America. "The study of man and his work," a definition he has often given for anthropology, became even at this early time an interest second only to that of ichthyology. From 1875 onward his work was mainly along anthropological lines. The previous year he had been an assistant in the Kentucky Geological Survey and from 1876 to 1879 he was assistant to the United States Engineers in surveys west of the One Hundredth Meridian. It was at this time that he made his first contribution to the knowledge of the prehistoric ruins in southern California, Arizona and New Mexico. Later the Pueblo cultures were to be the subject of elaborate researches undertaken under his direction.

From an original suggestion of Professor Othniel C. Marsh of Yale University to his uncle, George Peabody, in London, the three Peabody Museums at Yale, Harvard, and in Salem came into existence. In 1866, Peabody gave the sum of $150,000 for an endowment of a "Museum and Professorship of American Archaeology and Ethnology in connection with Harvard University." Jeffries Wyman served as Curator of the new museum until his death in 1874. Following him, Asa Gray was appointed Curator *pro tem,* as he felt he could not assume permanent charge of the Museum. Putnam, who had been associated with Wyman in his archaeological field researches and had been doing independent work in mound exploration, was appointed, in 1875, Curator of the Peabody Museum, a position he held for thirty-four years, when he became Honorary Curator, and from 1913 to the time of his death he was Honorary Director.

Starting in 1866 with a case of fifty specimens housed in Boylston Hall under Wyman the museum had grown rapidly. Putnam became Curator about the time that the Peabody gift had grown to the figure set by the donor and in 1876 the first part of the Peabody Museum was begun. The rapid growth of collections made it necessary to enlarge the building in 1889, and in 1914 Putnam's tireless efforts made a further addition possible, thus completing the building, as originally planned by Agassiz, by joining the geological section of the University Museum. Putnam happily lived to see the fruition of his hopes. For almost forty years, in spite of numerous other positions, his main interest was the Peabody Museum. Soon after his appointment as Curator, he included scientific monographs in his annual reports; in 1888 he started the "Papers of the Peabody Museum," and in 1896 the series of "Memoirs" was added. Not the least of Putnam's abilities was his success in raising money for scientific investigations. His first public appeal for aid was in 1882 when he was able to undertake extensive archaeological research in Ohio and among the Indians of the Plains. In 1890, Charles Pickering Bowditch became the Museum's most generous benefactor, and the following year marked the first of a series of archaeological expeditions to Central America which has continued almost down to the present time. The able direction of Putnam is seen in the conduct of these expeditions and his editorial ability is demonstrated in the long series of Papers and Memoirs published under his Curatorship.

The endowment given by George Peabody to found the Peabody Museum included a Professorship in American Archaeology and Ethnology, but nothing was done about the appointment until June, 1885. The Harvard Corporation then appointed Putnam Peabody Professor. Confirmation by the Board of Overseers was not given until 1887. This postponement deprived Putnam of the honor of being the first Professor of American Archaeology in an American university. During the previous year Daniel G. Brinton was appointed Professor of American Archaeology and Linguistics in the University of Pennsylvania. Three years later the Division of American Archaeology and

Ethnology was established at Harvard and the Hemenway and Thaw Fellowships were established in 1891. In spite of severe opposition, mainly from Charles Eliot Norton, the name of the Division of American Archaeology and Ethnology was changed in 1903 to the Division of Anthropology. The courses offered now embraced many fields outside those of American Archaeology and Ethnology. Putnam saw very early that the Museum should not limit itself to American Archaeology and Ethnology, but should widen its scope to include collections from the whole world. By the deed of gift, this was possible. Today the Peabody Museum, one of the great anthropological museums of the world, stands as Putnam's greatest monument. This zeal for the new cause brought him ample reward in students, a notable increase in collections and research funds, and money for building and equipment purposes.

The archaeology of the Mound Area of the United States became Putnam's special interest, and his long-continued excavations at the Turner Group and at Madisonville, Ohio, brought this area to the notice of the scientific world. For almost the first time, methods of exact measurements coupled with topographical maps, sections, and other scientifically determined data were employed in American archaeology. The old haphazard methods of digging for objects gave way to a knowledge of the context of specimens—the manner of their occurrence. His field notes are models of recording archaeological data. Through Putnam's effort, the famous Serpent Mound in Ohio was bought by Harvard University, later to be turned over to the Ohio Historical Society for permanent preservation. One of the first to recognize the importance of removing archaeological monuments from the vandal "pot-hunter," he played a part in the laws for the preservation of several of these works of ancient man, especially some of the most important of the "cliff dwellings."

In connection with his work for the Peabody Museum and for the other institutions with which he was connected he personally conducted or directed archaeological work in thirty-seven different states, in addition to extensive expeditions to Central America and Mexico and more limited investigations in South

America, Canada and Europe. His energy and enthusiasm, coupled with great perseverance, are markedly shown in these several fields of investigation. Perhaps the best example was the search for geologically ancient man on the edge of the glacial gravels at Trenton, New Jersey. In spite of opposition and much discouragement, he insisted the work here should be kept up. For thirty years he had one or more men constantly at work investigating this site for the possible presence of early man. The work at Trenton was supplemented by similar lines of investigation in California. He lived and died with the firm conviction that man lived on this continent in glacial times. He felt that he had been able to prove this with the human femur found in the glacial gravels in New Jersey. It is a pleasure to note that excavations later carried on by the American Museum of Natural History serve to substantiate Professor Putnam's thesis and the accuracy of his observations. There is perhaps no other single point of investigation within the whole field of archaeology that has been so long-continued and so carefully carried out as this work at Trenton. However much one may differ with Professor Putnam as to the subject in question, no one can fail to admire the persistence of his search for the truth.

Throughout his life, Putnam gave few formal courses of instruction. Following Agassiz's example, he much preferred to have his students meet him in the laboratory for informal instruction. During the first years of anthropology at Harvard, he often gave lectures on subjects in which he was especially interested in regular courses offered by his colleagues. A list of his students would contain names on the teaching staffs of many colleges and on the staffs of practically all the anthropological museums of the country. One of the great reasons for his success was his remarkable and unselfish interest in the work of his students. The sincere and sympathetic personal contact was always present. He never turned anyone away who showed any interest and any aptitude for anthropology. He always found a way to provide for a poor student, even if the means had to come out of his own pocket. He never was too busy to listen and he was always filled with an abundant optimism. As a public lec-

turer, he was at his best. He appeared most successfully before many different scientific societies, associations and clubs. He never entirely left out of consideration the fact that he was a pioneer in the new subject. With an infectious enthusiasm and seeing the great opportunities for anthropological work in America, he realized the necessity of student recruits in this field and money to support the work.

Attending his first meeting of the American Association for the Advancement of Science in Toronto in 1857, he very rarely missed a session of this organization. Becoming Permanent Secretary in 1873, he retained this office for twenty-five years. As the one permanent official in this "great mother organization of American associations of learning," the policies he inaugurated and kept alive made a very deep impression on scientific research in America. On laying down this ungrateful task, he was made President in 1898. Dr. W J McGee, at a dinner in Cambridge in celebration of Putnam's seventieth birthday, said in part: "I desire especially to signalize one feature of Professor Putnam's career which seems to me distinctively national and permanent in character. Throughout the entire formative period of the American Association for the Advancement of Science, Professor Putnam was Permanent Secretary, practically the sole continuous officer of the Association; and his efforts in its behalf were ceaseless and constantly successful. This, too, was the formative period of American science. Now what the Association (which I regard as our most typical and most useful scientific institution) would have become without Putnam—who can say? Certainly his impress is large; certainly its character and standing must in no small measure be credited to him. And what American science would have been without the Association—who can say? Certainly its character and prestige are the greater because of the work of the Association and because of Putnam's efforts in its behalf. It is doubly pleasant for one coming from another center of thought to acknowledge the debt of the nation to a man and to an institution that have done so much toward preparing the way for that

larger knowledge of humanity made necessary by the modern view of nature in which the resources loom so large."

His appointment as chief of the ethnological section of the World's Fair at Chicago in 1892 marked an epoch in American anthropology. One of his first moves in his new office was to have the structure in which his department was housed called the "Anthropological Building." This, so far as I know, was the first official use of the word "Anthropology" in the New World. Among those whom he gathered around him as his co-workers at Chicago was Dr. Boas, then docent in Anthropology at Clark University, whom he made his first assistant. As part of the out-of-door exhibit at Chicago, Putnam had full-size models of several of the Maya buildings in Yucatan. The interest aroused by the collections and exhibits brought together in Chicago resulted in an immense impetus being given to the study of this subject in colleges and to the creation of anthropological museums. The "cabinet of curios" now gave way to scientific collecting. As a direct outcome of the Chicago Fair, the Field Museum was started. Its conception was due, in large part, to Professor Putnam, and one of his students was the first Curator of Anthropology in this institution.

Another result of the work at the World's Fair was an opportunity for Putnam to organize the Anthropological Department at the American Museum of Natural History in New York, where he was Curator from 1894 to 1903. He called Boas to New York to assist him. Under their joint leadership, some of the most far-reaching anthropological investigations ever inaugurated were undertaken, more especially the Jessup North-Pacific Expedition. The Hyde Expeditions to the Southwest were also carried out at this time and Putnam spent several seasons in the field surrounded by his students and directing the excavations.

Never too busy to lend a hand at organizing and extending anthropology, in 1903 he was called to the University of California to organize a department and a museum on the invitation of Mrs. Phoebe Hearst. Here he was Professor of Anthropology and Director of the Anthropological Museum from 1903 to 1909, afterwards Professor Emeritus, spending a part of each

year in California. Professor Kroeber writes in this connection, "He was then sixty-four years of age; but in spite of the handicaps of remoteness during a large part of the year, he threw into his California service all the habitual vigor and unremitting care of his youth, plus the seasoning of his mature experience. The writing of his hand remains in the broad outlines of this institution as visibly as in those on which he had fashioned before. In spite of ill health in which there became manifest before long the symptoms of the disease to which he was ultimately to succomb, he continued to the utmost of his strength his activities in California, until his retirement at the statutory age of seventy in 1909."

His ability as an organizer seen at Cambridge, Chicago, California, and New York, was evident during his earlier years. He played a major part in starting *The American Naturalist* and *Science*. There should also be mentioned his share in the establishment of the Archaeological Institute of America in 1879. The others on this organizing committee were Charles W. Eliot, Charles Eliot Norton, Alexander Agassiz, William Endicott, Jr., W. W. Goodwin, Augustus Lowell, Martin Brimmer, T. G. Appleton, W. E. Gurney, Henry P. Kidder and C. C. Perkins. The traditions of Classical Archaeology were broken for the first time and American Archaeology, perhaps reluctantly, was invited to enter the fold. This new departure was due in great part to the personality and to the enthusiasm of Putnam.

Recognition outside his own field came tardily, and, it must be confessed, rather grudgingly in some cases. Starting with no academic background in the usual sense, and in a new field of learning, he had to meet the criticism of the classical archaeologists who saw nothing in American archaeology and in Indians. There were, at first, few affiliations with other branches of science. He had to wrench the study of "early man and his work" out of the hands of the amateur and of the dilettante and place scientific foundations under a structure which, at first, had only very vague outlines. His accomplishment was great in direct proportion to the many discouragements he met in his early endeavors. He received an honorary A.M. from Williams

as early as 1868, but Harvard did not recognize his arduous labors in behalf of anthropology until 1892, when he was made an honorary member of the Phi Beta Kappa. Two years later he received an honorary Doctor of Science degree from the University of Pennsylvania. In 1896 he was given the Cross of the Legion of Honor from the French Government, and in 1903 the Drexel Gold Medal from the University of Pennsylvania, an honor shared by Flinders Petrie, Evans, and Hilprecht. He had been selected as the recipient of an honorary degree from Oxford in 1912 at the meeting of the International Congress of Americanists, but his health prevented him from taking the trip to England. He was elected to the National Academy of Sciences in 1885. He was also a member of the American Academy of Arts and Sciences, and the American Philosophical Society, the Massachusetts Historical Society, and a large number of other learned societies of the United States. He was an honorary or corresponding member of scientific societies in London, Edinburgh, Paris, Brussels, Berlin, Stockholm, Florence, Rome, and Lima.

His seventieth birthday in 1909 was made the occasion for presenting him with a *Festschrift* to which his friends and colleagues contributed. The subjects of the papers offered in his honor covered the whole field of anthropology, including physical anthropology, archaeology, ethnology, sociology, religion, folklore, and linguistics. A bibliography of Putnam's writings is also included in the volume. Professor C. H. Toy presided at the celebration and President Eliot, representing Harvard University, was the first speaker. He touched upon the many difficulties which surrounded the early attempts of Professor Putnam in establishing the teaching of anthropology in Harvard University, the way these difficulties were overcome, and the gratifying results of Professor Putnam's work. He spoke of the growth of the Peabody Museum from small beginnings and the development of research connected with the Museum, and drew a parallel between the pioneer work of Asa Gray in botany and Professor Putnam in anthropology.

Professor Franz Boas, through whose initiative the volume was undertaken, was the second speaker. He read a long list of the learned societies which had sent felicitations to Putnam on this occasion, including various learned bodies of the United States, South America, England, Sweden, France, Germany, and Italy. Dr. Boas said in part: "I consider it a great privilege to be allowed to express to you the good wishes of your many friends—those here assembled, and of the many more who could not join us tonight to do honor to you. Many years of enthusiastic work, not only in your chosen field of science, but also in behalf of every subject that has appealed to your generous sympathy, have knit firmer bonds between you and your wide circle of friends. I wish to give expression particularly to the feelings of those who are working with you toward the advancement of anthropology. When we look back upon the growth of our science during the last forty years, three names stand out prominently among American anthropologists—your own, that of John Wesley Powell, and that of Daniel Garrison Brinton. We owe to you the development of steady, painstaking methods of field research and of care in the accumulation of data; not detached from the ends sought by Powell, not without ideas as to their interpretation, but looking forward steadily and firmly toward a goal that cannot be attained in a few years, nor in a generation—that must be before our eyes all the time, and the attainment of which demands our whole energy. No trouble has been too great for you in the pursuit of this aim; and to your facility of creating enthusiasm among half-willing friends of science, anthropology owes much of what it is. We can hardly turn to one of the great centers of anthropological research without finding that its very existence, or at least much of its work, is due to your inspiring personality. It is not for me to speak of the work that you have built up in Harvard University, but I have been witness to the success of your inspiration in Chicago and in New York. Without your unselfish work for the World's Fair, the Field Museum of Natural History would not be what it is. You laid the ground for the anthropological work of the American Museum of Natural History in New

York, and the periods of its great anthropological activity were when you were there. In the Far West, in California, anthropological work has grown up under your influence and under your watchful eye. If I were to count the institutions that have benefited from your wise counsel, I might go on without end. Much as you have thus done for the advancement of anthropology, we should not do justice to you if we were to forget the personal influence that you have exerted upon all those whose good fortune it has been to work with you. Through your kindly interest in his scientific work and in his personal welfare, you have succeeded in making every one of us your warm personal friend. It has been our desire to give permanent expression to our feeling of gratitude to you; and it seemed to us that this could be done in no better way than by presenting you with a book containing some of the results of the investigations of your former collaborators and of those who continue work in your special field of research. Your many friends here and abroad, personal friends, patrons of science, institutions in whose behalf you have labored, and your colleagues and collaborators have joined in the preparation of the book that I have the honor to present to you in their behalf. It is meant to be a token of our friendship and gratitude, and a witness for all time to come, not only of the important services that you have rendered to science, but also of the bonds of friendship that you have established between yourself and your younger colleagues."

Putnam's writings number more than 400 papers and reports, about equally divided among those devoted to natural history, to archaeology, and to scientific administration. His own research in archaeology is shown in reports upon shell mounds in Maine and Massachusetts, the mound builders of Ohio and Wisconsin, the use of copper by the American aborigines, the human deposits in the caves in Kentucky, the geological antiquity of man in New Jersey and in California. Perhaps the most far-reaching papers are those on conventionalization in the ancient art of Panama, and symbolism in the ancient art of America. These served as the point of departure for many studies in primitive art. His largest work is the report in *Archaeology,*

forming Volume VII of the Wheeler Geographical Survey, in which he reviewed the pre-history of California.

It was not as a writer, however, that Putnam will be remembered. He gave freely of his time, his advice and ideas to others who profited largely therefrom. He was a painstaking and most efficient editor of the publications of the different societies and museums of which he was the head. The annual reports for twenty-five years of the American Association for the Advancement of Science, and for thirty-four years of the Peabody Museum are models of excellence. His fame rests upon the organization and development of the new science of anthropology. Not only in Cambridge, but in New York, Chicago, and in California, he has left a never-to-be extinguished impress upon anthropological institutions and the teaching of anthropology. Students came to him from all parts of the country and his great enthusiasm sometimes failed to separate the able from the mediocre. In the early days of anthropology, recruits were few, and perhaps there was no other way to gain students for this study. This does not mean that he had no discrimination, but he felt he had to use the material available. Excellent men also came to him and his kindliness, unselfishness, encouragement, and direction were exhaustless. His interest was more than purely scientific; he wanted to know his students and their backgrounds, and he shared in all their interests. Professor Kroeber wrote in 1915 in this connection: "Professor Putnam's helpful influence on men, especially young men, at the outset of their scientific careers, was no less profound than his accomplishments for science through his upbuilding of institutions. He never encroached on their freedom, met even abnormalties of thought with patient tolerance, and if he requested heavy drafts of their time, he was always and instantly ready to reciprocate with equally generous measures of his own hours. Above all, he looked upon them as friends; they were human beings in need of encouragement and assistance, not mere thought machines to be perfected and turned adrift. Each and every one of his students he helped. Their existence for him did not end with their departure from the university or exploring

camp. His most valuable aid frequently began only then, and if occasionally the relationship thus established atrophied, instead of becoming warmer with the passage of years, the fault was never his and the regrets were on his side. It is no exaggeration to say that at least half of the anthropologists of the country today owe not only counsel, but their first professional recognition to the influence of Professor Putnam. In the vast majority of cases they admitted and continued to appreciate this debt toward their Dean, whose hours in his later years were frequently cheered by visits that bore testimony to the unwavering friendship and respect of former pupils and assistants.

"In all his relations with men, Professor Putnam showed the same high qualities of sincerity, helpfulness, and unassuming modesty, charged at all times with a genuine and practical benevolence. The humblest of those dependent upon him regarded him with affection; and it was precisely the qualities which on the one hand caused janitors and doorkeepers at institutions he had long left to mourn his death, which on the other hand accorded him the respect and the hearing of men of affairs and endowed him with an unvarying influence upon his boards of trustees."

As the then President-elect of Harvard, A. Lawrence Lowell, said at the Seventieth Anniversary, Professor Putnam had enjoyed the very unusual opportunity of opening an entirely new field of research and of developing a new science which had come to be of such great importance, an opportunity not given to many.

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John F. Hayford

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XVI—FIFTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

JOHN FILLMORE HAYFORD

1868-1925

BY

WILLIAM H. BURGER

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1931

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CONTENTS

JOHN FILLMORE HAYFORD

1868-1925

BY WILLIAM H. BURGER

FOREWORD

In February 1928 President T. H. Morgan of the National Academy of Sciences invited the writer to prepare a memoir of John F. Hayford, a member of the Academy, who died in March 1925. This invitation was accepted, and since that time, as occasion permitted, material was gathered a little at a time. Owing to the writer's ill health and the pressure of his duties as Professor of Civil Engineering in the College of Engineering at Northwestern University, there were periods when very little time could be spent on the work, but at last it is near completion in so far as the writer considers possible without an undue expenditure of time and energy. He believes that it is fairly complete as to events and dates, and it is hoped that the results indicate in a measure the extreme admiration which the writer had for the man whom he had known through a period of twenty-six years, in camp life on field surveys, in connection with various scientific societies and organizations, in the atmosphere of a great educational institution, and in his home. It is further hoped by the writer that this memoir is a fitting, final tribute to the man whom he had known as Chief, Councillor, Colleague, and above all, as Friend.

WILLIAM H. BURGER.

Evanston, Illinois, March 15, 1931.
School of Engineering, Northwestern University.

PREPARATION OF MEMOIR

In the preparation of this memoir the writer has made use of the large collection of letters, documents, diaries, reprints of publications, pamphlets, and other material which were placed in storage at the College of Engineering at Northwestern University by Director Hayford and which were kindly turned over to

the writer by his successor; also permission was granted to examine the fifteen years' accumulation of correspondence in the Director's file; also his son, Maxwell, transmitted such material along this line as was found at his home at the time of his death, and who has also reviewed the manuscript of this memoir. He is also indebted to the relatives, friends, and colleagues who so kindly furnished assistance in response to requests, and especially for the information furnished by Major William Bowie, Mr. Hayford's successor in the U. S. Coast and Geodetic Survey, and finally, to Mr. C. H. Swick of the same bureau for his review of the manuscript.

Added to the above is the fact that the writer had enjoyed an almost uninterrupted acquaintance with the Director for twenty-six years, first as a member of his force in field and office work in Washington and later as a member of his faculty at Northwestern University. During these twenty-six years the writer's fields of work were closely related to some of the fields in which the Director worked, and, therefore, on many occasions the writer was invited to participate in examination or discussion of the investigations carried on by the Director, and many times was able to assist in computations and in other forms of service.

While the duty of collecting the material and trying to form a presentable memoir from it has been a rather heavy burden on account of the continued ill-health of the writer combined with his college and other work, it has been throughout a most pleasant duty to work on this a final tribute to him whom the writer esteemed so highly.

The writer first formed the acquaintance of Mr. Hayford on July 21, 1899, when he reported as Aid in the United States Coast and Geodetic Survey for duty in the Geodetic Division under Mr. Hayford's charge and was assigned to a party doing precise leveling in Nebraska. On his return from this field party in October of the same year, the writer was assigned to duty in the Computing Division and remained in Washington during the winter working on precise levels under Mr. Hayford's direction. As the writer had been selected to take charge of a field party in the following spring, a party to use the newly de-

signed Coast and Geodetic Survey level, it was but natural that many conferences were had with Mr. Hayford, and thus an acquaintance began which soon ripened into a friendship which lasted for more than twenty-six years.

It was during that winter that Mr. Hayford formed a small class, composed of two Aids (of which the writer was one) and one man from the Computing Division. This class met for one evening each week at Mr. Hayford's home in Northwest Washington for the purpose of studying and receiving instruction in the theory of least squares and adjustments of observations. The writer at the present date wonders if those three men had any conception of what demands they were then making upon the time of a man who was under the heavy burden which now appears to have been his lot at that time, and further wonders if they fully appreciated the greatness in a man who could and would find time to lay aside his larger and more important duties, or his chance for an evening's study or amusement, to assist some of the lesser men in his department. Perhaps if they had thoroughly understood the situation their thanks would have been much more pronounced. To place self in the background when others needed help seems to have been one of the great characteristics of Hayford's nature, for this he was ever ready and willing to do.

BIOGRAPHY (Summary)

For general purposes there is here given a rapid summary of the more important dates and events in the life of Dr. Hayford. Some of these are dealt with at greater length elsewhere in these memoirs.

Born May 19, 1868, at Rouses Point, New York.

Attended country schools, Rouses Point High School two years, Detroit High School two years.

Entered Cornell University, College of Engineering, 1885.

Graduated, degree Civil Engineer, 1889.

Appointed Computer, U. S. Coast and Geodetic Survey, June 22, 1889.

Served in Tidal Division until December 15, 1890.

Transferred to Office of Standard Weights and Measures, and served there until July 20, 1891, when he went as recorder on the Holton, Indiana, base line for three months. Returning to the office of Standard Weights and Measures, he remained there until the end of December 1891, and was then transferred to the position of Aid in the field force of the Survey.

Assistant Astronomer, U. S. and Mexican International Boundary Commission, February 1892 until January 1894.

Returned to the Survey as Aid and was promoted to Assistant in 1894.

Summer 1894 in Alaska on astronomical work in connection with boundary survey.

Married Lucy Stone, October 11, 1894.

September 1895 to April 1898, instructor Civil Engineering, Cornell University.

July 9, 1898, appointed Expert Computer and Geodesist in the United States Coast and Geodetic Survey.

May 3, 1899, became Inspector of Geodetic Work.

Appointed Assistant in the field force, 1899.

Appointed Inspector of Geodetic Work and Chief of the Computing Division in 1900 and served in that capacity until October 1909.

Delegate with O. H. Tittmann, representing the United States at the Budapest meeting of the International Geodetic Association, 1906, and also at the London and Cambridge meeting of 1909.

Elected Director, College of Engineering, Northwestern University, October 1908, to take up the duties in September, 1909.

Elected to National Academy of Sciences, April, 1911.

Chairman, Commission of Engineers, Costa Rica-Panama Boundary Arbitration, October 1911 to November 1913.

Appointed member National Advisory Committee for Aeronautics, 1915.

Appointed member of Commission to study Panama Slides, November 18, 1915.

Granted honorary degree Sc.D. from George Washington University in 1918.

Special war work in Washington on Airplane and Navy instruments.

Awarded Victoria Medal by the Royal Geographical Society of Great Britain, 1924.

Awarded Chanute Medal, Western Society of Engineers, 1925.

Died at Evanston, Illinois, March 10, 1925.

DISTINCTIVE HONORS

1894

Awarded Fuertes Medal of Cornell University for paper in Transactions of the Association of Civil Engineers of Cornell University. "An Account of Certain Field Methods Used on the Survey of the Mexican Boundary in 1892-93."

1918

Degree Sc.D. George Washington University. Awarded for scientific work done in the Government service.

1924

Awarded the Victoria Medal of the Royal Geographical Society of Great Britain for the establishment of the theory of isostasy.

1925

Awarded Chanute Medal by the Western Society of Engineers for superiority of paper published in the Journal during the year 1924.

1925

Name perpetuated by National Geographic Society in the naming of "Mount Hayford" in Alaska.

ANCESTRY

John Fillmore Hayford was born at Rouses Point, Clinton County, New York, May 19, 1868. He was the son of Hiram and Mildred Alevia (Fillmore) Hayford, and a descendant of William Heiford who came from England about 1668 and was an early resident of Essex and Old Norfolk, Massachusetts; from him the line descends through his son John Heiford of Braintree, Massachusetts, who married Abigail Albins; their son John and his wife Lydia Pierce, who lived near Farmersville, New York; their son John (first to spell his surname Hayford) and his wife, Thankful Phinney; thence through their son John who was the great grandfather of the subject of these memoirs. This ancestor with his wife Elizabeth Riley came from Brainbridge, Connecticut, in 1800, built a log house in the dense forest, and commenced clearing to make way for farm crops. This farm has been held by the family since that date, and it was here that John was born. His grandparents were Asel Hayford and Esther Cobb.

John's father's father died at the age of eighty, his father's mother at the age of seventy-two, and his father at the age of sixty. His mother's father died at the age of eighty-seven, his mother's mother at the age of twenty-six, and his mother at the age of seventy-nine.

John had four brothers and three sisters. One brother died in early youth and one sister died in infancy. His other brothers and sisters survived him: Hiram C. Hayford (1930), Rouses Point, New York; Horace Hayford (1930), Pasadena, California; Benjamin Hayford (1930), Waukesha, Wisconsin; Mrs. Emily (Hayford) Coates died in 1928. He lived with this sister during his high school days in Detroit; Mrs. Clara (Hayford) McMurdie (1930), living at Kalamazoo, Michigan.

SCHOOLING

His father died when John was eight years old, and it was necessary for John during his boyhood to help on the farm be-

fore and after school. He received his first schooling in a one-room stone schoolhouse in School District No. 4, in the town of Champlain in which the farm is located. This schoolhouse was in a fair state of preservation when the writer visited Mr. Hayford's brother there in 1907. His brother Hiram, writing in 1930, reports that John ran away from home when he was four years old. The whole neighborhood was out looking for him and he was finally located at the schoolhouse one mile away. He had told the teacher that he would like to come to school. Thus early did he show his desires for mental activity.

When thirteen years old he entered Rouses Point High School, but stayed there only two years, when he went to Detroit to attend high school. While in Detroit he lived with his sister, Mrs. Emily Coates. It is reported that he walked nearly six miles every morning delivering the Detroit Free Press to help pay his expenses. This helped to keep him physically fit and to build up the vigorous body which was to be called upon to stand so much in later life.

Of his life as a student in high school the writer has been unable to obtain much information. Evidently it was of a high order for John was selected to give the Commencement Oration at the graduation exercises of his class held on June 24, 1885, the subject of his oration being "College Influence." It is interesting here to note that another on the same program was Robert P. Lamont who defended the affirmative on the commencement debate, "Should Foreign Immigration be Restricted?" The class consisted of twenty-four girls and seven boys. It is significant that the part taken by Hayford and Lamont in this high school program should point so conclusively to their future lines of work,—Hayford along scholastic lines to become a renowned scientist and educator, and Lamont along political lines to become Secretary of Commerce under President Herbert Hoover.

After graduating from the Detroit High School, Mr. Hayford was awarded a state scholarship at Cornell University from the Assembly District which included Clinton County, New York. He held this scholarship for the four years while he was at Cornell. He entered Cornell in the fall of 1885 and was

graduated in 1889. His brother Hiram is authority for the statement that after the first year John practically worked his way through college by waiting on table in a boarding house in Forest Home, a little town about four miles from Ithaca, and in that way paid his board. He also took care of a horse and the furnace of one of the faculty members of the college. There is among his papers a record of his having obtained financial aid from Cornell University in the form of loans secured by notes which were paid off in 1892.

Of his college life while a student at Cornell the writer is indebted to Anson Marston, Dean of Agriculture and Mechanic Arts of Iowa State College. Dean Marston writes under date of March 2, 1929:

"I became acquainted with Director Hayford in 1885, when we both entered the College of Civil Engineering of Cornell University, Ithaca, New York, as freshmen. He took from the first a prominent place as a student in the Civil Engineering College, and to some extent in university athletics. I remember that in our freshman year he electrified the watching crowds in one of the track events by running from behind under the arm of a tall, lanky senior athlete who was considered to be one of the best runners at Cornell at the time. I remember that Hayford later became interested in fencing, taking part in public contests.

"From the beginning of his college course, Director Hayford showed a great interest in pure and applied mathematics. At that time Cornell University was conducting a 'Lake Survey' every spring for two weeks, in which the juniors and seniors conducted triangulation and did topographic and hydrographic work in surveys of the 'finger lakes' of central New York, one after the other. We did our work on Lake Canandaigua, with headquarters at a summer hotel before the season opened. I remember that at the end of our junior year, Hayford was captain of a stadia party and that he gave a great deal of attention to the methods of adjusting and using the extremely antiquated transit with which the party was supplied.

"At that time the work at Cornell was organized in three 'terms,' fall, winter, and spring, corresponding to the 'quarters' of our present-day university practice in institutions which do not use the semester system. In the spring term of our senior year, Director Hayford inveigled another classmate, now Dean F. E. Turneaure at the University of Wisconsin, and myself into taking an elective course in which the object was the adjustment of the triangulation data obtained in these surveys, accumulated for several years. My own part in the course was mainly to operate a calculating machine, but Director Hayford took an intense delight in planning and directing the work in all its features in detail, and it was a fitting prelude to his after work on various U. S. boundary surveys, and with the U. S. Coast and Geodetic Survey.

"It is my recollection that during the last three years of our work at Cornell, ten engineering students, including Hayford, Turneaure and myself, became the owners of a second-hand sail boat, which afforded us much recreation in patching, painting, and even sailing it occasionally. Professor H. N. Ogden of Cornell University was another of these ten men. He was from Maine and our most accomplished sailor. To this boat we gave the name 'Secant,' and upon one occasion she upset and thus became a 'co-secant'."

In connection with athletics while at Cornell his son Maxwell is authority for the statement that Hayford won the Intercollegiate Mile Championship for 3 years, and placed second in the mile walk. He received many medals for his races, some of which are now in his son's possession.

From other sources it has been gleaned that Mr. Hayford also took part on several occasions in college dramatics and even started to learn to play the violin. Evidently while in college he led the same kind of intense life which was so characteristic of the subsequent years. Judging from the stories which he told, college life and classroom work must have been full of keen enjoyment for him. This was one of his predominating traits, always to get the fullest from every phase of life or activity into which he entered, whether mental, spiritual, or physical.

In addition to his work at Cornell along surveying and mathematical lines, he took great interest in the physical sciences, and under Professor I. P. Church laid the foundation for the great progress he made in later years in his studies and investigations on isostasy and stream flow and other related scientific works.

In Dean Marston's recollections of Mr. Hayford's life at Cornell, mention was made also of Dean Turneaure. The acquaintance of these three men continued throughout Mr. Hayford's life, and they met often and corresponded much, and he spoke of them with highest praise. He had also a close friendship with another classmate of the class of 1889. This was James S. Stone, a brother of Lucy Stone to whom Mr. Hayford was later married. The class of '89 seems to have been a remarkable class. At least four members of it attained "Who's Who in America," three as Deans of Engineering Schools, and one, Professor Carpenter, as Professor of Experimental Engineering, all renowned as scientists and educators.

UNITED STATES COAST AND GEODETIC SURVEY

Tidal Division

It was but natural that on graduation from Cornell he should seek employment along those lines in which he seemed particularly gifted, lines along which he had shown aptitude during his college days; and on June 22, 1889, he accepted service with the United States Coast and Geodetic Survey at Washington, as computer. Thus began his relation with the Survey, a relation which was to bring him such renown, and in a reciprocal way add to the prestige of this bureau of the government. On reporting for duty he was assigned to the Tidal Division. Part of his work in this division was of a general routine nature, and part along special lines of investigation. He made a special study of the Ferrel machine, of Harmonic Analyzers, and frequently discussed with Mr. E. G. Fischer regarding a new tide-predicting machine in the Survey. His work in this division gave rise to his first published works: (a) "Mean Range and

Improvement on the Tidal Machine," (b) "Use of Observations of Currents for Prediction Purposes," and (c) "Comparison of the Predicted with the Observed Times and Heights of High and Low Waters at Sandy Hook, N. J., during the Year 1889," all published by the Survey as shown in the Bibliography at the end of these Memoirs.

Office of Standard Weights and Measures

He served in the Tidal Division until December 15, 1890, when he was transferred to the Office of Standard Weights and Measures, a division of the Survey which was later to become the National Bureau of Standards. Here he served under O. H. Tittmann, who later became Superintendent of the U. S. Coast and Geodetic Survey. His work in the Office of Standard Weights and Measures, as in the Tidal Division, was on the general observing and computing incidental to the routine work of the division, but he also had an opportunity to do some original work, the results of this being given in the Survey Report for 1892, appendix No. 10, "On the Least Square Adjustment of Weighings." In July his work in this Office was interrupted for a few months when he was sent on an assignment to Holton, Indiana, returning upon its completion to the Office of Standard Weights and Measures where he remained until the end of December, 1891.

Holton Base

The first field work in which Mr. Hayford took part was when he was sent to Holton, Indiana, July 20, 1891, to act as recorder on the measurement of the Holton Base Line, under Assistants O. H. Tittmann and R. S. Woodward. He served in this capacity during the measurement of the base until October 12, returning to Washington on October 27, 1891, the intervening time being spent on leave of absence, visiting in Detroit, Rochester, N. Y., and Rouses Point, N. Y.

At Holton Base, Mr. Hayford formed an acquaintance with one of the men of the base party, and between them began a friendship which was destined not only to affect later his reputa-

tion as head of the Division of Geodesy in the U. S. Coast and Geodetic Survey, but in a great measure to affect the geodesy of the United States and of the entire world. This acquaintance with Jasper S. Bilby ripened into an abiding friendship which lasted until the death of Mr. Hayford in 1925. When Mr. Hayford went to the work on the United States-Mexican Boundary, Mr. Bilby was employed as general helper in his party and he has served continuously to date with the Coast and Geodetic Survey, with rare periods omitted, engaged for the most part in the Geodetic Division. Since about 1900 all of the major reconnaissance and signal-building has been in his charge and it was under Mr. Hayford's régime that special recognition was given him by conferring upon him the official title of Signalman, the first to be thus honored. In speed and economy of operation his work has had a distinct bearing upon the phenomenal success attained by the Coast and Geodetic Survey in triangulation and base work. He is the designer of the Bilby Steel Tower now being used with great success. Recently he again received official recognition by being given the title of Chief Signalman, this position having been especially created for him. The writer has had the pleasure of working with Mr. Bilby on many occasions, and believes that as an expert on reconnaissance and signal-building Mr. Bilby stands unrivaled in the world.

In December, 1891, Mr. Hayford was transferred to the position of Aid in the field force of the Survey, and during January, 1892, a considerable part of his time was spent in Washington in preparation for the work on the United States-Mexican Boundary survey to which he had been appointed.

MEXICAN BOUNDARY

Owing to the fact that difficulties had arisen regarding the exact location of the boundary line between the United States and Mexico a convention was concluded between the two governments at Washington, July 29, 1882. Its provisions were not carried into effect before the date of its expiration, and another convention between the two governments to revive and continue the same was concluded February 18, 1889.

In accordance with Article V of this convention, the International Boundary Commission, organized November 17, 1891, was to carry out the provisions of the convention to mark the boundary from the Pacific Ocean to the Rio Grande. The United States commissioners appointed by the President of the United States were Lieut. Col. J. W. Barlow, Corps of Engineers; First Lieut. D. D. Gaillard, Corps of Engineers, and Mr. A. T. Mosman, Assistant, Coast and Geodetic Survey. To carry out the work of the American Commission several parties were organized, and in February, 1892, J. F. Hayford, with the title of Assistant Astronomer, was placed in charge of one of these, namely, the astronomical party for determining latitude and azimuth. Mr. Hayford reported directly to Commissioner Mosman who, also, under the title of Astronomer, had general supervision over the astronomical work along the line.

The work of the astronomical party began in Washington in December, 1891, and on January 22, 1892, Mr. Hayford left Washington for El Paso via New York and Galveston, arriving at El Paso on February 3, and on February 12 the astronomical party went into camp near El Paso, in conjunction with the other survey parties, fully equipped and prepared for work. Mr. Hayford's immediate party consisted of himself as astronomer and observer; James Page, computer; J. S. Bilby, general helper, and a cook and two teamsters. Their equipment consisted of one spring wagon, one baggage wagon, and, when necessary, one water-tank wagon.

All of the observations for latitude, azimuth, time, and magnetic declination on the entire line from El Paso to the Pacific Coast were made by Mr. Hayford in person. He also executed some detached secondary and tertiary triangulation, and ranged out two hundred and thirty miles of line.

So far as the writer knows, this is the first position held by Mr. Hayford where he was essentially chief of a field party and completely responsible for the successful completion of the work assigned. When one considers the magnitude of the work to be done and the difficulties under which the parties worked it

is difficult to realize that this was his first responsible assignment. At this time he was only twenty-four years of age.

The nature of the country traversed had a most important bearing upon the organization of parties and methods adopted for the survey. The whole region is essentially arid, and natural features and conditions were such as arise from its arid character. Volcanic peaks are frequent and in many places the lava had poured in a great flood over the plains, making the desolate region even more desolate and forbidding and furnace-like, as the lava rock catches and throws out again the fierce heat of an unclouded sun.

The greatest difficulty was to secure water, and many times it was necessary to haul water fifteen to twenty miles to camps, sometimes fifty miles, and in one case over one hundred miles. The summer temperature was very severe. During the second summer of the survey, in crossing the Yuma Desert, temperatures rarely fell as low as 90° throughout the night, and reached 104° for four or five hours nearly every day, and on one day indicated 118° in the shade. The only shade in general was that furnished by the tents, vegetation offering none, and the noon day rest found no cooling shade.

To add to the difficulties under which the parties labored in crossing the Yuma and Tule deserts, nearly half of the helpers deserted on this their first experience of the desert, and practically all of those remaining gave out at the end of two or three days, leaving the instrument men to perform their work unassisted.

This work called for the endurance of the most rugged of pioneers, the undaunted courage of the explorer, while the operations involved represent one of the highest types of work demanded from the scientist.

By direction of the War Department the Commission during its operations was provided with a military escort of about fifty enlisted men with three officers to accompany the expedition as a protection against Indians or other marauders.

The determination of each of the three non-parallel sections of the boundary, namely, the meridian position and two western

sections, consisted in the prolongation of the line from its determined azimuth at one end. The tracing of the parallel of latitude sections involved frequent astronomical stations. These were established at successive distances of about twenty miles. Observations were made to determine latitude and azimuth, and tangents to the parallel at each astronomical station were laid out and lines prolonged to the next station. Intermediate points on the boundary were determined by computed offsets.

A detailed report of this work by Mr. Hayford is given in the "Report of the Boundary Commission upon the Survey of the Boundary between the United States and Mexico West of the Rio Grande, 1891-1896," pp. 62-168.

Besides furnishing him much material for his text-book on *Geodetic Astronomy,* it brought to Mr. Hayford the much-prized Fuertes Medal of the College of Civil Engineering of Cornell University, awarded to him about 1894. In regard to this medal Director E. E. Haskell of the College of Civil Engineering in 1911 wrote to Director Hayford saying, "There seems to be no record in this College in regard to the award." On April 20, 1911, Director Hayford replied, "I have no definite record showing the basis of the award of the Fuertes Medal to me. My memory is that it was awarded for the paper entitled 'On Certain Field Methods Used on the Survey of the Mexican Boundary in 1892-1893,' published in the Transactions of the Association of Engineers of Cornell University, 1894, pp. 55-83."

Mr. Hayford was engaged upon the Boundary work from December, 1891, to December 26, 1893.

The Commission, in reporting upon the work to the Congress of the United States, stated in regard to the work of the four principal engineers in charge of divisions (Mr. Hayford being in charge of the astronomical section):

> "They all brought to bear upon their duties rare intelligence and excellent training, and throughout the difficult and arduous work devolving upon them were ever willing . . . and to them is due large credit for the successful completion of the survey."

The writer, in his long intimate acquaintance with Mr. Hayford, has listened many times to descriptions of this work, de-

scriptions replete with tales of heroism and endurance of the men engaged upon it and which would constitute an epic if but known to the world, but accepted by them as part of the day's work.

"GEODETIC ASTRONOMY"

Mr. Hayford made use of much of the material gathered on the Boundary Survey in his book on *Geodetic Astronomy,* published in 1898. His own personal copy, in which he had made marginal notes since 1898, is in the writer's possession and needless to say it is a much-prized volume. It contains a wealth of material on observing and computing time, latitude, longitude, azimuth, and least squares, written in such a clear, comprehensive manner as to make it stand near the top of the list of published works on this matter. Unfortunately, it did not have the ready sale which a more popular text-book would have received, and when the plates were melted by the publishers a few years before Mr. Hayford's death, and with his permission, he remarked to the writer that this book had "netted him fifteen cents for each hour spent in its preparation."

On completion of his work with the Boundary Commission, Mr. Hayford returned to his regular duties in the Coast and Geodetic Survey, and was promoted to Assistant in the Survey in 1894.

ALASKA WORK

On April 19, 1894, Mr. Hayford left Washington under an assignment to join the U. S. Coast and Geodetic Survey steamer "Hassler" at Seattle for work in Alaska under Assistant John F. Pratt, commanding the steamer. During the summer he was stationed near Pyramid Harbor, Anchorage Bay and Tanja Inlet, Alaska, and was engaged in making longitude and other astronomical determinations in connection with the survey of the Alaska boundary. He returned to Washington September 8, 1894, to resume his regular work as Assistant in the Survey.

MARRIAGE

On completion of the work at Holton, Indiana, we find that Mr. Hayford spent one week at Detroit visiting with his sister,

after which he went to Rochester and Charlotte, N. Y., to visit at the home of James S. Stone, a classmate of his undergraduate days at Cornell University. It was during this visit that he became engaged to Lucy Stone, the sister of James. Her father was William T. Stone. She also had another brother, Walter King Stone, who has attained fame as an illustrator. This engagement ended in a wedding on October 11, 1894, soon after his return from field work at Anchorage Bay, Alaska. They spent their wedding trip visiting Niagara, Detroit, Chicago, Janesville and Madison, Wisconsin, and arrived in Washington, November 1, 1894.

As to his work during the latter part of 1894 and the first half of 1895, the writer has not been able to learn, but from items that have been discovered it is judged that he was employed in Washington on general work at the Survey office. These items also indicate that it was not a period of all work and no play, for we find evidence that he and Mrs. Hayford made good use of their stay in Washington by making frequent trips to see the nearby points of historic interest; also they took advantage of the many good operas and other forms of amusement afforded by the winter in Washington. Even the lighter forms of entertainment were not neglected, for we find them joining the Mask and Wig Club and participating in amateur theatricals.

INSTRUCTOR AT CORNELL

In September, 1895, Mr. Hayford resigned his position as Assistant in the United States Coast and Geodetic Survey to accept a position as instructor in Civil Engineering at Cornell University. His duties at Cornell were teaching Mechanics, Practical Astronomy, Land Surveying, and Descriptive Geometry, and guiding the laboratory work of certain students in connection with Mechanics and Hydraulics. The statement contained in the previous sentence occurs in a penned copy of a letter written February, 1898, and addressed to Professor William H. Burr, Columbia University, New York City. It is the only reference which the writer has been able to discover in regard to the work at Cornell, and in the twenty-six years of inti-

mate acquaintance he very seldom heard Professor Hayford refer to this phase of his life, and even then the sentiment expressed was such as to lead one to believe that he had not been very happy in his work there and would not care to say much about it. The writer's belief is that his teaching load was so heavy that he could find little time to apply himself to lines of work in research, etc., which were more to his liking. We do know that he responded most enthusiastically to the invitation to return to the Coast Survey.

COAST AND GEODETIC SURVEY, 1898-1909

As stated elsewhere in this memoir, Hayford's correspondence was exceedingly heavy and he was very careful in preserving it. But of his personal correspondence previous to his coming to Northwestern University the writer has not been able to locate much. Evidently it had been packed and placed in some place unknown to or forgotten by his relatives. Among the very few letters preserved in their original envelopes is one on which he had written in red ink, "Letter which brought J. F. H. to the C. & G. S. in 1898," and is dated January 1, 1898. In this letter Henry S. Pritchett, then Superintendent of the Coast and Geodetic Survey, writes to Hayford at Cornell:

"I should be glad to have a talk with you in regard to certain plans which are likely to be carried out in the Coast and Geodetic Survey and which will, if put into execution, give opportunity for permanent work in geodesy along lines which promise results of great interest and value."

Under date of June 2, 1930, Dr. Pritchett wrote to the writer regarding Hayford's entry into the Coast Survey:

"When I went there in '97 and began to study the institution and its organization, one of the first requests I made was for some one who would come as an understudy to Mr. Schott, with the understanding that he would, in a short time, become chief of the division. Mr. Schott was still vigorous but getting quite old. I was led to think of Hayford because of the admirable work he had done in the Survey and because of some of his papers. I never knew him until he came to the Coast Survey.

At that time he was at Cornell. I got him to come down to Washington and talk the whole matter over with me and was delighted at the enthusiasm and interest with which he took up his task."

Evidently the position offered to Hayford at this time was subject to U. S. Civil Service Commission examination, for Hayford took an examination for Expert Computer and Geodesist on June 7, 1898, and on July 6 was rated by the Commission as here shown:

Ability and experience in discussion of geodetic problems and administration of computing work......	100
Publications in the line of Geodesy, mathematics and astronomy	100
Positions held in professional life.................	100
Mathematics	95

It is not known whether he had any competitors in this examination, but it certainly would have been difficult for any one to have exceeded the score made by Hayford. It was a score which practically assured him of the appointment.

As Cornell University was at that time organized on the term instead of the semester basis, we find that Hayford's work at the university came to a close at the end of March, 1898, and he moved with Mrs. Hayford and their son Walter to Washington.

He evidently had been given temporary work in the Survey pending his regular appointment, for on May 11, 1898, he was given instructions by the Superintendent to confer with Professor J. H. Gore of the Columbian University in reference to his proposed pendulum observations at the Survey office and to assist Professor Gore to the extent of observing time for these observations. Also this temporary appointment must have been to the position of Expert Computer and Geodesist, for on July 1, 1898, such an appointment was continued until July 15. On July 9, 1898, the Civil Service Commission certified to his probationary appointment as Expert Computer and Geodesist, and Hayford took the oath of office on that same date.

He was assigned by the Superintendent to report to the Assistant in Charge of the Office for duty in the Computing Divi-

sion, taking the position of chief computer. His duties were to have general charge of the other employees in the division, under the direction of Assistant C. A. Schott. Hayford at this time was only thirty years of age, and comparatively young to have charge of a division in which, at times, there were many men considerably older than he was, and men with considerably more experience. How well he carried on is fully shown by the work of the Computing Division and Geodetic sides of the Survey in the following years, and thus was added one more to the long train of scientists which starting from Hassler did so much to bring the U. S. Coast and Geodetic Survey to its high position in the scientific world.

It was in July of this year that he completed his text-book on Geodetic Astronomy on which he had been engaged since August, 1896, and on which he had put over 1200 hours of work. Although working in the Geodetic and Computing Divisions at this period, Hayford evidently found time to work in other fields than the strictly geodetic, for in December, 1898, he published in *Science* a lengthy article on "The Limitation of the Present Solution of the Tidal Problem," and in March, 1899, *Terrestrial Magnetism* published his monograph, "Is There a 428-day Period in Terrestrial Magnetism?" The latter contains, probably, Hayford's first attempt at utilizing, for purposes of research, the stresses that are set up in the material of the earth.

On July 29, 1898, Hayford was instructed by the Superintendent, by authority of the Secretary of the Treasury Department, to attend the meeting of the American Association for the Advancement of Science at Boston in August, 1898, to officially represent the Coast and Geodetic Survey.

On May 3, 1899, his designation was changed to that of Inspector of Geodetic Work, and in the same year he was given an appointment as Assistant in the field force of the Survey. His rating as Inspector of Geodetic Work was granted upon the recommendation of Charles A. Schott, the "Grand Old Man" of the Coast Survey, and who for more than fifty years was identified with the work of the Coast Survey. At this time Schott was in charge of the geodetic work and of the Computing Divi-

sion; he was reaching an advanced age and realizing that his life's course was about completed, gave up the supervision of the geodetic work in 1899 in order to devote the remaining years to the completion of the transcontinental and eastern oblique arcs of triangulation in the United States. How well he reckoned may be judged from the fact that the Great Transcontinental was published in 1900 and the manuscript for the Eastern Oblique Arc completed in 1901, and on July 31, of the same year Mr. Schott received his "final call" at the age of seventy-five.

In 1900 Hayford, having served as Schott's understudy in the Computing Division for two years, became Inspector of Geodetic Work and Chief of the Computing Division in the Survey. He was the first man to have this double designation granted to him. Schott, as Chief of the Computing Division, was also the directing agent of the geodetic work, but so far as the writer has been able to learn, both titles had never been officially conferred upon him. In this position Hayford had charge, under the Superintendent's direction, of the operations of triangulation, leveling, astronomical determinations and gravity work, from the formation of the plans for field work to the publication of the results, with supervision over the field as well as the office operations.

During 1898-1909 very notable advances were made by the field parties under Hayford's direction in the methods of precise leveling, primary (now called first-order) triangulation, telegraphic longitude determinations, in gravity work, and in the methods of determination of the figure and size of the earth from geodetic operations. In each case a much higher efficiency was secured than formerly, the unit cost of the work being greatly reduced, while in many cases an increased accuracy was obtained. While these were the most important operations carried on under his direction, there were many minor ones in which he had a helping hand in improving, or modifying, so that the operations were carried on more economically or more conveniently. These various phases of his labors will be treated at greater length elsewhere in these memoirs.

During this period Hayford was rapidly improving his mathematical skill and his knowledge of the applications of mathematics and engineering to geodesy. Dr. Pritchett wrote concerning Hayford's work, "In my efforts, during the three years I was Superintendent of the Survey, to reorganize in large measure the scientific work I found Hayford a most practical and willing adviser. He had unusual mathematical ability coupled with the quality of clear judgment—a rare combination."

While still Inspector of Geodetic Work he spent considerable time in the preparation of the Fourth and Enlarged Edition of Coast and Geodetic Survey treatise on "Determination of Time, Longitude, Latitude and Azimuth," which was published in 1899, and which forms the manual for field operations in these subjects. Many important additions were made to the Third Edition, compiled by C. A. Schott, Assistant. The principal additions were (1) an account of the method of determining differences of longitude by transportation of chronometers as used by the Survey, (2) an account of the micrometric method of determining azimuth by means of a theodolite or transit furnished with an eye-piece micrometer, (3) considerable new matter bearing upon the relative magnitude of errors to be expected from various sources, and upon the economics of observing. This manual was published one year later than Hayford's *"Text-book on Geodetic Astronomy,"* from which some of the material for the manual was derived.

Although Hayford was continually at work studying to improve existing methods and devising new methods to make the operations in his departments more efficient, when once these methods proved efficient he proceeded to gather together all the good features and place them in convenient form for use by his office or field force. These collections were then issued in the form of "Standard Instructions" and they constituted the basis on which the work should be carried on. In this way the chiefs of parties had definite goals set them in the results to be attained and they seldom had to grope about for the general methods of procedure. This did not mean that they could not use their own initiative. In case any man thought he had a better way

and would produce evidence, Hayford was always willing to consider such evidence and gladly made the changes in the instructions if evidence warranted it. He was always ready to listen to any proposal from his subordinates in this respect. Results, and these within specified limits of accuracy and with a consideration of economy in expenditure of time and money involved, were the prime factors he considered. This led to a fine *esprit de corps* in his department and the men gladly gave of their energy, both mental and physical.

It is difficult to tell, in an orderly and continuous narrative, of Hayford's activities from 1898 to 1909. In addition to carrying on the routine work of the two divisions of which he was in charge, his many researches led him into so many fields that to portray them in a suitable manner necessitates that they be considered separately, even though there is an overlapping in the time element. Consequently, his various activities are dealt with in the separate accounts which follow, and which bring the story of his life and works up to November 1909, when he resigned from the United States Coast and Geodetic Survey to accept the position of Director of the College of Engineering at Northwestern University, Evanston, Ill.

Precise Leveling

When Dr. Pritchett took charge of the Coast and Geodetic Survey in 1897 the matter of precise leveling was, to quote his words, "in a bad way," and "the instruments we were using at that time had certain drawbacks." He appointed Mr. Hayford chairman of the committee to take up this matter, and largely due to him a régime of precise leveling was introduced which both greatly reduced the expenses of field work and improved the instrumental conditions.

In the winter of 1898-99 the committee made a most exhaustive study of the leveling executed in the United States by the Coast and Geodetic Survey, as well as by other organizations of this government, and also by foreign governments, and of possible errors and of their sources. One of the things discovered was that there existed a systematic error which was traced

to the unequal thermal expansion of the leveling instruments which had been employed. As a result of Mr. Hayford's studies he made recommendations to Mr. E. G. Fischer, at that time in charge of the Instrument Division of the Survey, that a level should be designed which would not be seriously affected by temperature. There is some question as to whom credit should be given for the design of this level. In a letter written April 11, 1911, to Professor W. K. Hatt of Purdue University, Mr. Hayford says, "I was responsible for fixing the general features to be embodied in the instrument and Mr. E. G. Fischer, the Chief of the Instrument Division, made the design." The level which was the outcome of the work by Mr. Hayford and Mr. Fischer and now known as the "U. S. Coast and Geodetic Survey Precise Level" was constructed in the shops of the Survey under Mr. Fischer's supervision early in 1900. The writer had the good fortune to be one of two men to whom was assigned the first field work with these new levels. It has been in constant use for over thirty years by the Coast Survey, to the practical exclusion of all other forms of precise levels, and has been used by other organizations in this country and to a great extent by geodetic organizations in other countries. It is generally recognized as the most efficient instrument for leveling of high precision that has ever been designed. Lallemand, the great French exponent of hypsometry, once made the statement that "with its use practically all of the errors of precise leveling exist *outside* of the instrument."

The most important features of this instrument and methods adopted in the precision leveling organized by Mr. Hayford were (1) Use of invar to as great an extent as possible to minimize temperature changes; (2) Adoption of the dumpy form with low center of gravity, giving relatively few parts to get out of adjustment, and high stability; (3) Single micrometer screw under the eye-piece of the telescope affording rapid manipulation and ease of holding the bubble centered even under difficult conditions; (4) The use of a reading device which enables the observer to see the bubble clearly at any time without moving his eyes from the eye-piece, and (5) A firm mounting of the level

vial nearly in the line of collimation, and in such a position that it is unusually well separated from rapid changes in temperature and, therefore, with the result of securing an unusually stable relation of the vial and telescope.

Before the introduction of this level the rate of progress was less than sixty miles per month. Recent work, which is of a much higher grade of accuracy, shows an average of about one hundred miles, and one observing party recently completed one hundred and forty-eight miles of progress, or more than three hundred miles of single line in one month. The cost of the operations in precise leveling has decreased in about the inverse proportion as the speed per mile of progress.

On January 23, 1906, in an address before the Washington Society of Engineers where he spoke regarding the level, after six years' experience with it, Mr. Hayford said, "This experience shows that such rapid and economical leveling has been accomplished with this instrument and methods as to prove that the same type of instrument and a similar method, if applied to leveling of the grade and accuracy ordinarily done with the wye level, would give much more rapid and much less costly work than the wye level ever can give." And then he issued this challenge, "Any engineer who will consider carefully all the available facts of this experience will find in them a standing challenge to show why he should not consign his wye-level to the junk-heap."

Length of Triangle Sides

In the older schemes of triangulation it seems to have been the practice in selection of stations to obtain practically the longest lines possible. Mr. Hayford's studies led him to the conclusion that this caused the triangulation to become excessively costly and slow. From various considerations he concluded that the extreme lower limit should be about five kilometers, but that this extreme lower limit should be avoided. On the other hand he concluded that from a standpoint of accuracy there was no advantage in using extremely long lines. According to his instructions to parties, the general considerations upon

which they should base their decisions were: (1) The cost of the work, including reconnaissance, signal building, and angle measurements, should be economical, and (2) there should be a sufficient number of accessible stations established to serve the immediate needs of the survey, and also to leave points useful to engineers; they should use the most economical length of line, namely, that length which would give the maximum of usefulness with the minimum of cost.

Strength of Figure Formula

The success of triangulation depends in large measure upon the intelligent investigations made in the reconnaissance. These investigations include the selection of the strongest and most feasible figures for the triangulation as affecting the economy of the operations to follow. The shape of the triangles constituting the figures must be taken into account. As the shape of a triangle is determined by its angles, the angles, therefore, must be taken into consideration.

Up to the year 1903 the general criterion which the man on reconnaissance kept in mind was that no angle should be less than 30° in the main scheme for primary triangulation. This often entailed considerable trouble to the reconnaissance party, and made difficult revision of points already selected, thus affecting the speed and economy of the operations. Mr. Hayford made a study of the strength of figures used in triangulation by applying to them the principles deduced in Least Squares, and among the general conclusions drawn by him was the fact that the 30° criterion was not necessarily a good one in all cases. He propounded the proposition that the triangles of a chain of figures be tested, this test to be based upon the magnitude of the *distance* angles used in computing the lengths from base line to base line and he proposed the now well-known strength of figure formula.

In February 1903, during one of the periods between field seasons when the writer was attached to the computing division of the Coast and Geodetic Survey, this problem was given to him by Mr. Hayford with instructions to study the methods

and to prepare the necessary tables to make it easily applicable to field use. It was first applied in actual field work by the writer in conjunction with Signalman Jasper S. Bilby while engaged on reconnaissance for primary triangulation in South Dakota in 1903; and it proved so successful that practically all reconnaissance is now executed in accordance with this method. By using this method the field officer on reconnaissance can, after having obtained rough angles for the triangulation figure under consideration, in a few minutes of time decide whether the figure is within prescribed limits of accuracy. It is safe to assert that it has very materially helped to produce more rapid and more economical reconnaissance without a lowering of the accuracy of the triangulation.

Base Measurement

Prior to 1900 all of the base lines used on arcs of first order triangulation, with only three exceptions, had been measured by using metal bars. The use of the steel tape had not received full sanction by those engaged in base-line work. There was a division of opinion among scientists as to whether or not the required degree of accuracy could be obtained with the tape. This doubt was natural with the older scientists for they had been trained in the school where bars were recognized as the prime requisite for base apparatus. Immediately after Mr. Hayford assumed charge of the Geodetic Survey he made a study of the measurements with tapes in this country, and with long wires in other countries, and became convinced that the long steel tape could be used in place of bars on base measurements provided these measurements were made at night. The coefficient of expansion of the material of the tapes was so great, and the uncertainty in the determination of the temperature of the tape by means of the attached thermometers was so large that it was believed by Mr. Hayford that the tape measurements should be made at night, as then the temperature of the tape and of the attached thermometers would be practically the same. Tapes had been experimented with at the Holton Base, Indiana, on which work Mr. Hayford had acted as recorder in

1891. He, therefore, had had some field experience in their use and also with base bars of the older type.

Mr. Hayford, with commendable caution, did not at once eliminate base bars, but decided to make a careful field comparison of the results obtained with base bars, 50-meter steel tapes and 100-meter steel tapes. This was done during the measurement of nine bases along the 98th meridian during the latter part of 1900 and early in 1901 with Mr. A. L. Baldwin in charge of the measuring party. A detailed report of this great accomplishment in the comparison of base measuring apparatus is contained in Appendix 3 of the Survey Report of 1901 by A. L. Baldwin, with a preface by Mr. Hayford.

On the base measurements along the 98th meridian there were used the Eimbeck base bars which had been developed for the measurement of the Salt Lake base, and the 50-meter and 100-meter steel tapes. Each base was divided into approximately three equal sections. One section was measured with the base bars, another with the 50-meter steel tapes, and the third with the 100-meter steel tapes. Each part of a base was measured at least twice in opposite directions. A kilometer of each base was used for an intercomparison of all the apparatus used on the base. From this test kilometer a true comparison of the tapes and of the base bars was obtained. The field party was also required to standardize the base apparatus at the beginning and end of the field season by using the iced bar designed by R. S. Woodward. This standardization was done in the field under conditions approaching as closely as possible in every respect those encountered during the base measurements proper.

The results of these tests indicated that the speed with which tape measurements could be made was more than two and one-half times as rapid as the bar measurements and the cost of measuring was about one-third of that with the bars, all work being executed in such a manner as to keep within a requisite degree of accuracy, indicated by a probable error of one part in 500,000. The accuracy with which the bases were measured gave about equal results with the tapes at night and

with the Eimbeck bars, while the operations were carried on much more conveniently with the former.

As a result of these comparisons on the 98th meridian, the Coast and Geodetic Survey adopted the 50-meter tape as the standard length for base work. To the student of the progress of science and of engineering this campaign will be interesting because it was planned and carried out in the spirit of engineering rather than of science, whereas in the past the reverse had been true as a rule. The engineer accepts past experience as his working material, decides upon the results which it is desired to secure, and then selects such methods as will secure those results with the minimum expenditure of time and money. The previous statement seems to embody Mr. Hayford's formula for all of the work planned by him, and to it is due in large measure the success attained by parties working under his direction.

Late in the past century, Professor Charles Guillaume of Paris discovered an alloy of nickel and steel (later called Invar) which has a very low coefficient of expansion. It was found that this could be rolled into wires and tapes, and Mr. Hayford soon recognized the possibility of using invar tapes in the measurement of primary base lines. On his recommendation the Coast and Geodetic Survey purchased a number of 50-meter invar tapes. The experimental work with these tapes, prior to their being taken to the field, was done by Assistant O. B. French, and these tests showed that the invar alloy had sufficient strength and elasticity for base measuring purposes, and so Assistant French was assigned, in 1906, to the measurements of six bases with them.

As in the case with the steel tapes, so here again a good field comparison was made in the case of the invar tapes. Assistant French measured the six bases with both invar and steel tapes, and the results showed conclusively that the invar measurements, made in daylight, frequently in bright sunshine, were even better than the measurements with the steel tapes at night. Since these successful tests of the invar 50-meter tape in 1906, there has been practically no improvement in base apparatus. Invar

tapes are now generally recognized as ideal for the work, and it is difficult to see how any improvement over them could be made.

Triangulation

Shortly before Mr. Hayford took charge of the geodetic work of the Coast and Geodetic Survey, the great transcontinental arc of triangulation along the 39th parallel had been completed, as well as an arc of triangulation along the Pacific Coast from San Francisco Bay to the Mexican boundary. The eastern oblique arc from New Orleans to Maine was practically completed. Their computations were being rushed to completion by Charles A. Schott, "The Grand Old Man of the Coast Survey," as a final and fitting close to his fifty years of service with the Survey. The year 1901 saw the completion of these computations, and in July of the same year Mr. Schott received his "final call."

The data for the stations composing these arcs were found to be of such great value in the surveying and mapping of the interior of the country, and for other purposes, that there was an urgent demand that similar arcs be extended over the whole interior of the country. To do so seemed a formidable undertaking, for the progress of a single observing party on first-order triangulation was approximately from eight to fifteen stations a season of approximately six months, or a progress along an arc of triangulation of from sixty to one hundred miles.

Mr. Hayford carefully studied the triangulation methods of this and other countries, and found that the efficiency with which the work was done here was quite equal to the best that was obtained abroad. However, his studies led him to believe that our methods could be improved upon, and he spent much time in studying methods and apparatus in detail, and consulted with many men on the force, especially with Mr. E. G. Fischer, Chief of the Instrument Division, in regard to apparatus, and with Mr. Jasper S. Bilby, at that time a signalman in the Survey, with the result that a number of radical changes were proposed and tried out in the ensuing years.

Signal Lamps

The development of the heliotrope at the time that Mr. Hayford took charge of the work was well advanced and was admirably suited for observations during afternoons when sunshine was available, but even on the best of days a few hours only could be utilized for observing. Night lamps of several kinds had been used, but with inconsiderable success, especially on moderately long lines. This led to the development of the signal lamp, which was first used in 1902 on the triangulation in Kansas southward along the 98th meridian. This consisted of an acetylene bicycle lamp to which was attached a condenser lens. In the middle west, where the atmospheric conditions are generally favorable, these lamps were effective to distances as great as 35-40 miles. The period of observing was thus increased from two or three hours a day to from ten to twelve hours by the inclusion of night observations, and it was proved that night observations were made more rapidly and with practically the same degree of accuracy as those made in the daytime. A few years later a larger acetylene lamp, such as was used at the time on automobiles, was modified as a triangulation signal lamp. The larger lamp proved to be very effective on lines of triangulation ranging from 30 to 75 miles in length. The writer has often used these larger lamps in tandem, one above the other, during murky weather, where it was impossible to catch the faintest glow from a single lamp, thus saving long, long periods of waiting.

Signal Towers

During this same year (1902) the style of observing tower was radically changed by improvements in design and methods of erection by Signalman Jasper S. Bilby, resulting in a great saving in time and strength of structure. Also the program of operation planned by Mr. Hayford on this work was a rather marked change from the older programs. The principal features of the 1902 organization and methods employed were:

1. Two parties which erected the wooden towers needed to elevate the instrument, heliotrope, and signal lamp to such

heights that the sides of the triangles were clear of obstructions.

2. The employment of two observers, one on each side of the arc of triangulation.

3. The permanent employment of light-keepers who would remain at a station and direct the light from the heliotrope or lamp toward the observers. Communication between observers and light-keepers was effected by employing the Morse Code, flashes of light being used to transmit the signals. Movements of the various groups were thus expeditiously ordered, with a consequent saving of much time.

4. The completion of the observations at a station in the main scheme of the triangulation during a single observing period, afternoon and night, if practicable to do so. Prior to the season of 1902 it was generally believed in this country and abroad, that the angles of the triangles should be observed on a number of different days in order to have varying weather conditions: it was believed that by following this method, the accuracy of the observed angles would be much greater.

The 1902 season was a marked success. While the previous record for a single party during any one season was 15 stations, with a maximum distance along the arc of 70 miles, there were 75 stations in the main scheme completed in 1902 and the progress along the arc was 444 miles. This was a far-reaching and epoch-making accomplishment in triangulation. Following 1902 other parties had greater accomplishments in number of stations per unit of time for a single observer and length of arc completed, yet the 1902 project marked the first great step forward in the application of engineering principles to first-order triangulation.

The great improvements made in the triangulation methods of the Coast and Geodetic Survey gave much promise of extending the first-order triangulation net over the United States in a reasonable number of years, for surveying, mapping, and many other uses. It was rather noteworthy that, up to the 1902 season, there had been accomplished only 5300 miles of

first order triangulation. The first observing had been done under the immediate direction of Ferdinand R. Hassler, the first Superintendent of the Bureau, in 1816, eighty-six years before. Since the beginning of the season of 1902, there have been executed by the Survey nearly 23,000 miles of arcs of triangulation.

The writer hesitates to close this account of the accomplishments of the work of the Survey without a brief sketch of the part played by Signalman Jasper S. Bilby. In the execution of the reconnaissance for triangulation, base-lines, and signal-building, Mr. Hayford and his successor, Major Bowie, were most fortunate in having a man on the Survey of the caliber of Jasper Bilby, a man who in his chosen field stands supreme in the world. Practically all of the reconnaissance and signal-building in first-order triangulation since 1900 has been executed by him or under his immediate direction, and to him is due, in large measure, the phenomenal mileage in first-order triangulation accomplished by the Survey.

Angle Measures

The writer is unacquainted with the part played by Mr. Hayford in instituting changes toward improvements in methods of angle measurements, with the one exception of the ingenious program of observing devised by Mr. Hayford, having for its objective the elimination of the necessity of computing and applying a correction to the observed directions, due to "error of run" in the micrometers of the direction instrument. The program of observing also caused the mean value of any angle to be practically free from errors due to periodic errors of graduation of the circle in the theodolite. This program called for sixteen direct and reverse sets of observations of directions over each line of the main scheme, each direct and each reverse set to be referred to the same initial line wherever possible, and each set to have a varying initial reading on this reference initial line, these varying initial readings to be spaced at definite positions around the circle. This method has been used to the complete exclusion of the older method, where error of run was taken into account, and has greatly facilitated the resulting computations.

It is believed, but of this the writer is not sure enough to make a decided statement, that Mr. Hayford is also responsible for the introduction of the two sets of double parallel lines used in making the backward and forward readings of the micrometers of the microscopes of the direction theodolite, which has so materially aided in making the observations and resulting computations of the triangulation. These double parallel lines in the micrometers were first used in 1903-1904 in Texas and all of the theodolites thereafter have been so fitted.

Primary Azimuth

In order to safeguard and to check the computed azimuths as carried through the triangulation and to control these in the least square adjustment of the net, it is necessary that astronomical azimuths be observed at intervals along the system. Up to 1904 these azimuths were at times observed by an independent astronomical party after the triangulation had been completed. This to Mr. Hayford's mind was productive of extra expense and loss of time, necessitating a second party in the field with the probable chance that the observing tower and azimuth mark tower would have to be rebuilt, or at least repaired if still extant. Also he believed that the resulting azimuth would be more accurate and serve its purpose to better advantage if observed at the same time as the triangulation, under the same conditions, and over one of the actual lines of the triangulation net. This program was first carried out by the writer on the triangulation from Brown's Valley, Minnesota, eastward to Aitken, Minnesota. It proved so successful that subsequent parties have followed this method. By it the regular directions observations on the main lines of the triangulation are temporarily discontinued and the angle between Polaris and one of the lines of the main scheme of the triangulation is observed in exactly the same manner, using the same method of observations as in the main scheme, the only difference being in the number of observations required for the azimuth observations.

Whereas the limit of rejection of observations upon directions in the main scheme was five seconds of angle from the mean of the sixteen direct and reverse observations taken, in the azimuth work the limit of rejection was fixed by the probable error of a single observation, and the observers were instructed to make a sufficient number of observations to make it reasonably certain that the probable error of the mean was within certain specified limits. In order to obtain this required accuracy, it was seldom necessary to take more than the normal set of sixteen direct and reverse measures. The results of this method silenced the criticisms, made before its adoption, that azimuths could not be obtained with a sufficiently high degree of accuracy from the tops of the high towers, and especially without observing on two or more nights in order to introduce varying weather conditions. After the party became accustomed to this work, the delay occasioned by discontinuing the regular observations in order to obtain azimuth observations was seldom over an hour or two, and with but slight inconvenience to the triangulation party.

While the phenomenal success of the triangulation after Mr. Hayford took charge of the geodetic work is due in large measure to the men who accomplished it in the field, to Mr. Hayford should rightly go the credit for the adoption of the methods to be employed and for the manner of selecting the men to carry out the work. Being in responsible charge of the geodetic work, any failure on the part of the field force in carrying out any adopted program would immediately be thrown back as a criticism of Mr. Hayford's régime. On the other hand credit should be given his régime when work was carried on successfully, even though in large measure credit be given to those actually doing the work. Since many of the methods were rather radical changes from the older order, it was inevitable that criticism would arise. Mr. Hayford selected men for the work who had but few years' previous experience on the work and thus were comparatively without prejudice and were therefore free to try out any change proposed. How well this worked out in actual practice is shown by the results.

Astronomical Latitudes

The most important improvement that Mr. Hayford made in the determination of astronomical latitudes was to change the method of observing the same pairs of stars on more than one night. He was convinced from his investigations that the uncertainty in the star positions was greater than the uncertainty in the observations for latitude, hence he concluded that it would be better to use a larger number of pairs of stars and to make all of the observations on a single night, if practicable. In other words, he concluded that single observations on each of many pairs of stars was a more effective method of obtaining a high degree of accuracy than to use a limited number of pairs of stars with each pair observed on two or more nights. The Superintendent approved Mr. Hayford's recommendations in this respect, and that method is still being followed by the Survey.

In 1905 a large campaign of latitude observations was inaugurated by the Coast and Geodetic Survey on which the plan described above was first followed. The writer had the pleasure of being in charge of the field party carrying on this work and between 1905 and 1908 determined the latitudes at 63 stations in the United States, with an average of 1.9 nights of observations at a station in order to obtain the requisite degree of accuracy. In 1908 Assistant William Bowie, now Chief of the Division of Geodesy in the Survey, occupied seven stations and at only one station were observations made on more than one night. The instructions under which the parties operated called for a sufficient number of pairs of stars to be observed at one station to make it reasonably certain that the probable error of the result should not be greater than $\pm 0''.10$.

Another decided advancement in the operations of determining latitude was in regard to computing the micrometer value. Previous to 1905 the general method in use was to turn the micrometer box 90° and observe upon close circumpolar stars near culmination. Such observations consume time both in observing and in computing, and experience showed that they were subject to errors. A careful study was made by Mr. Hayford

of this part of the work and the Survey decided to adopt the method of computing the micrometer value from all of the latitude observations at a station, for it seemed quite evident that the value obtained in this way was more accurate than that determined from even three or four sets of circumpolar observations, each requiring an hour or more of time, and since the beginning of the 1905 latitude campaign no observations have been made on close circumpolar stars for that purpose.

The Transit Micrometer

A short time previous to the beginning of the present century Repsold, the great designer and maker of astronomical and other instruments, had designed what was called the "impersonal micrometer" for making observations of star transits by means of a movable transit line in the eye end of the observing telescope. Such a micrometer driven by clock work had been used many years before, but with little success. Repsold's design called for a hand driven movable line, and tests by the Prussian Geodetic Institute proved its superiority over the other methods of observing star transits. The prime object of the attachment was to eliminate the effect of the personal equation in making observations for time.

After Mr. Hayford assumed charge of the geodetic work he made a careful study of the possibility of using the impersonal micrometer on the transits of the Coast and Geodetic Survey for longitude work. While it had proved successful on the instruments at fixed observatories, the question to be decided was whether it could successfully be applied to the lighter portable transits in use by the Survey. At his request, Mr. E. G. Fischer, Chief of the Instrument Division of the Survey, purchased an impersonal transit micrometer from Repsold. He made certain modifications, simplified the apparatus, and made it more effective for use.

Very exhaustive tests of this improved impersonal micrometer, or "transit micrometer" as it is now known were made at the office of the Coast and Geodetic Survey under Mr. Hayford's direction. These tests were made on eighteen nights be-

tween March 15 and May 3, 1904, sixteen different observers taking part. These observers ranged from observers having much experience on longitude work to those having had no previous experience on any kind of field work. The results of the test showed that the relative personal equation between any two observers with the transit micrometer is so small as to be masked by the accidental errors of observation, and, therefore, it would be unnecessary to have exchange of observers on longitude work, a procedure that had up to this time been followed. The transit instruments of the Survey were accordingly fitted with transit micrometers of this improved type, and when used in actual longitude work proved to be most successful in facilitating field operations and in reducing the time and cost involved in making longitude determinations.

Standard Datum

Toward the close of the last century there had been completed in this country a number of arcs of triangulation, principally the great transcontinental arc along the 39th parallel of latitude and the eastern oblique arc. While all of these arcs had been computed on the same spheroid of reference (Clarke 1866), there was, owing to the detached character of many, no harmonious correlation between them. It was apparent, if these arcs were to serve their highest purpose, that they must be brought into close correlation with each other.

In the United States this was accomplished by the adoption of an initial point to which all of the connected arcs then existing were referred. Not only was an initial point selected (station Meades Ranch in Central Kansas), but a latitude and longitude for that station were adopted which would make the sum of the squares of the differences between the triangulation and astronomical latitudes and longitudes of a number of astronomical stations a minimum.

Upon examination of the data, however, Mr. Hayford found that the datum used for the triangulation in New England agreed so closely with the chosen ideal datum, that upon his recommendation the Superintendent of the U. S. Coast and Geodetic Sur-

vey in 1901 approved the adoption of the latitude and longitude of station Meades Ranch, as computed through the triangulation from the New England datum. By selecting this position for Meades Ranch a large amount of recomputation could be avoided. The foundation thus having been laid, Mr. Hayford, as Chief of the Computing Division of the Survey, started several members of this force on the long labor of recomputing and adjusting the triangulation of the United States upon this, the then designated United States Standard Datum.

This was a geodetic movement of far-reaching influence, not only to the United States, but also one of great importance to the North American continent. It placed the geodetic work of the survey upon one datum for the correct coordination of the geographic latitudes, longitudes, distances, and azimuths. From the scientist's point of view it furnished accurate correlation of data for a study of the figure of the earth, of isostasy, and for other related sciences. Since that time all of the triangulation of the United States has been computed on that datum.

Owing to the fact that the triangulation of Canada and Mexico coincided with that of the United States at many points along their common boundaries, the United States Standard Datum was adopted by those countries in 1913. It thus became a matter of international importance, and consequently its designation was changed to that of North American Datum, an event of great importance in the history of geodesy.

Dr. William Bowie, now chief of the Geodetic Division of the Coast and Geodetic Survey, in writing to Professor Hayford in regard to some publications of the Survey, on December 6, 1913, wrote the following: "The foundation for most of these publications was laid when you extended the U. S. Standard Datum throughout the triangulation net of the country."

The Standard Datum, as finally adopted, assigned to the station Meades Ranch the following position on the Clarke Spheroid of 1866:

Latitude	39° 13′ 26″.686
Longitude	98° 32′ 30″.506
Azimuth to Waldo.................	75° 28′ 14″.52

Gravity at North Tamarack Mine

In 1902 President F. W. McNair of the Michigan College of Mines, in an interview with the Superintendent of the U. S. Coast and Geodetic Survey, indicated that if matters could be arranged he would like to have the Survey furnish the necessary instruments and an officer skilled in their use to make some determinations of the value of the intensity of gravity at the North Tamarack Mine near Calumet, Michigan.

Such an arrangement was made with the Survey and with the mine owners, and Mr. Hayford was given the assignment to carry on the work with President McNair.

The regulation standard set of quarter-meter pendulums were used on this work. They were standardized at the base station in Washington in August before being taken to Calumet and standardized again in October after their return.

The observations were made at the North Tamarack Mine between September 9 and 20. Three stations were occupied. One was upon the surface. One was in the same vertical line as the surface station, and about 4600 feet below it. The third station was about 1200 feet below the surface station, and a few hundred feet out of the vertical.

The purpose of the observations was to determine the difference between the value of gravity at a point on the surface at the mine and two points beneath the surface, with a view to studying the vertical gravity gradient. Such a determination of the gravity gradient in connection with an estimate of the density of the materials between and near the stations would furnish a new determination of the mean density of the earth. It would be still more valuable, however, as furnishing a means of testing various theories as to the formation and present condition of the earth.

An account of this work was made by Mr. Hayford in an unpublished report to the Superintendent of the Survey in February 1904, and contains a most splendid discussion of errors and their effects upon the value of the intensity of gravity. In so far as the writer knows, this is the only case where Mr. Hayford was actually engaged in a field determination of the value

of the intensity of gravity, although he had engaged in pendulum work at the base station in Washington. It was probably one of the contributing causes which crystallized his efforts in starting the long campaign of gravity observation which in a few years was destined to add so many new determinations to the list of those executed in this country.

Deflection of the Vertical

After Professor Hayford had placed the triangulation of the United States on the United States Standard Datum, as explained elsewhere in this memoir, he was in a position to make a comprehensive study of the deflections of the vertical at many of the triangulation stations of this country. It is not known when this problem was first brought to Hayford's attention, but the following, quoted from a letter to the writer by Mr. O. H. Tittmann, formerly Superintendent of the Coast and Geodetic Survey, may solve the question. Writing under date of June 24, 1930, Mr. Tittmann says:

"It may be of interest to you to know that Helmert suggested to me the desirability of computing the osculatory surface fitting our extensive and homogeneous triangulation rather than to discuss meridians and parallels. When, as Superintendent, the opportunity came to me, I submitted to Hayford the problem of doing it, and of correcting the triangulation for deflection of the zenith. How well he solved the problem is known to all geodesists, and Helmert told me, after the publication of his results that he himself would have hesitated to undertake so extensive a computation."

As is well known, there is a lack of harmony between latitudes and longitudes determined by direct measurements of triangulation and those determined by astronomical means. These discrepancies were generally recognized as being due to the effect of the irregular surface of the earth on the direction of the plumb-line at the points where the astronomical observations were made, the astronomical observations being referred to the direction of the plumb-line at the point of observation. Any

disturbance of this direction necessarily affects the resulting latitudes and longitudes.

The problem of the deflection of the vertical is so intimately related to Hayford's two other monumental works, Isostasy and the Figure of the Earth, that it is almost impossible to separate them. Hayford attacked the stupendous work of computing the deflections of the vertical only after most careful study and consideration of the difficulties involved. His plan was approved by the Superintendent of the Coast and Geodetic Survey, who permitted the research work to be carried on in the Computing Division of the Survey. The rate of progress was mainly fixed by the fact that only a small amount of time was available in the Division, for the major duty of the Division was taken up in the general work of the Survey, and research work of this type could be but a small part of the activity of the Division.

Upon these three works, i.e., deflection of the vertical, figure of the earth, and isostasy, Hayford was engaged for more than ten years. As he states, "Attention has been given to the problem during hundreds of short periods of a few hours each, or even of a few minutes each, in the intervals between other duties."

In the first study of the deflection of the vertical reported upon to the International Geodetic Association at Copenhagen in 1903 there were available 246 station errors in latitude, 76 in longitude, and 152 in azimuth, all expressed on the United States Standard Datum and therefore comparable with each other. Hayford made computations to show what should be the theoretical values for the deflections of the vertical at the many astronomical stations that were connected with the triangulation, and devised methods for rapidly reading from maps what effect each topographic feature had upon the deflection of the vertical at any station. While studying the relation between the general features of the topography and the direction of the vertical in 1901, he reached the conclusion that one very efficient means of investigation would be the graphic method, and accordingly such a method was devised. By it the effect of all topographic irregularities within 4,126 kilometers of each astronom-

ical station were taken into account in studying the deflections.

An examination of tabulated deflections showed that there was a strong tendency for the deflections of the vertical over various regions of considerable magnitude to be of one algebraic sign. In other words, there was rather conclusive proof that there exist regional deflections common to large areas of the United States, as well as local deflections about a station. The most salient fact brought out by a study of the list of deflections was that there is a decided tendency for the deflections on both the Atlantic and Pacific slopes to be in such a direction as to indicate that they are due to an excess of mass beneath the oceans or to a defect of mass in the continent, or to both.

These facts led Hayford to a consideration of the theory of Isostasy which had been advanced some years before by geodesists and geologists, and also to a consideration of the Figure of the Earth.

Figure of the Earth and Isostasy

Mr. Hayford began the study of the figure of the earth about 1903. The report made to the Fifteenth General Assembly of the International Geodetic Association, held in Budapest, Austria, in 1906, gave an outline of the methods which he devised and was following in his investigations of the figure of the earth. In that report a statement was made as to the data used and how the computations of the topographic deflections were made. Topography was considered by Mr. Hayford to be the masses of the earth's material above sea-level and the deficiency of mass in tidal waters.

Professor Hayford's methods of attacking the determination of the figure of the earth formed a notable contribution to geodetic science. His use of the area method rather than the classic arc method was especially noteworthy. In the 1906 report to the Conference at Budapest the subject of isostasy was given consideration and it was shown that isostasy must be considered in connection with the determination of the figure of the earth. In this investigation reported upon, 507 astronomical determina-

tions were used, 265 of which furnished that component of the deflection of the vertical which lies in the meridian, and 231 stations furnished the prime vertical components.

The figure of the earth investigations and also the investigations in isostasy, involving the deflections of the vertical, were continued by Mr. Hayford, and again in 1909 he reported to the Sixteenth General Conference held at Cambridge and London the results of this further investigation involving 765 astronomical observations, this being an increase of more than 50 per cent over those used in the 1906 report. A complete report of this later investigation was published in "The Figure of the Earth and Isostasy, from Measurements in the United States," published by the Coast and Geodetic Survey, 1909, and in "Supplemental Investigations in 1909 of the Figure of the Earth and Isostasy," published in 1910. These two epoch-making reports of Professor Hayford were well received by the scientific world and led to his being elected a member of the National Academy of Sciences, the greatest honor that this country has to offer a scientific man.

Up to this time Mr. Hayford had been engaged for more than ten years in developing the ideas concerned in these investigations. While the work was in progress, various preliminary statements had been made by him in papers before scientific gatherings. A summary of the noteworthy features of the investigations, as given by Mr. Hayford, follows:

1. The investigation of the figure of the earth and of the reality of the condition called isostasy is based entirely upon observed deflections of the vertical in the United States.

2. No use was made in the investigations of determinations of gravity, for it was believed best to deal thoroughly with one phase of the investigation before taking up the other.

3. The area treated extends over a wide range in latitude and longitude—18° 50′ in latitude and 57° 07′ in longitude.

4. A large number of astronomical observations have been used.

5. All of the astronomical determinations are connected by a

continuous primary triangulation. The triangulation does not consist of separate and distinct belts of triangulation or arcs.

6. Unusual methods of computation have been used.

7. The effect of all topographic irregularities within 4,126 kilometers of each astronomical station have been taken into account.

8. The effect of possible distribution of densities beneath the surface of the earth corresponding to the condition called isostasy has been carefully taken into account and the existence of said condition established.

9. The investigations lead to values of the equatorial and polar dimensions of the earth, based on observations in a single country, the United States, which are of a very high degree of accuracy.

As stated by Mr. Hayford, every known device was utilized for reducing the time required for the computation of topographic deflections without allowing the accuracy to fall below the necessary high standard, and several new methods were devised by him to further the work. Very early in the investigation it was realized that it would be necessary to compute the topographic deflection for each station, and that the computation to serve its full purpose must extend to a great distance from the station. It was also realized that to make such computations by any method known to have been used before would be impossible on account of the great expenditure of time and money involved. It was necessary, therefore, to devise some new method of computation, or to modify old methods, so as to make these computations feasible. An ingenious method by the use of templates was devised by Mr. Hayford, and served the purpose admirably. By the method of computation used in this investigation, one computation for either a meridian or a prime vertical component of the deflection at a station, taking into account all the topography within 4,126 kilometers (2,564 miles) of the station, was found to take on an average the equivalent of 9.4 working hours for one computer.

Before proceeding to a study of the possible relation of the distribution of the sub-surface densities to observed deflections of the vertical, a secondary study was made to develop the extent to which the observed deflections of the vertical are related to the topography. This investigation was made by constructing contour lines of the geoid (or actual earth) graphically, starting with the observed deflection of the vertical as a basis. The problem was one of constructing the contour lines which will represent the relation of the irregular geoid to the regular ellipsoid of revolution known as the Clarke Spheroid of 1866, which is supposed to be in the position fixed by the adopted United States standard datum.

In order to carry on the investigation of the possible relation between the theory of isostasy and the deflections of the vertical, Hayford assumed that isostatic compensation is complete and uniformly distributed with respect to depth from the surface down to some unknown depth of compensation, which it was desired to find from the observations. He developed a series of mathematical equations showing the relation which he believed should exist, introduced the observed values of deflections, etc., and proceeded to make least-square solutions on several basic assumptions as to depth of isostatic compensation. A number of solutions were made assuming various depths of compensation, each successive assumption being influenced by the previously determined values. For Solution E (depth of compensation 162.2 Km.), solution H (120.8 Km.), and solution G (113.7 Km.), the sum of the squares of the residuals changed but very little, solution G giving the smallest, and it was considered the closest to the truth.

The two major results of this investigation are those given in Nos. 8 and 9 of the list of noteworthy features already stated in this article, namely, (1) the existence of the condition called isostasy was established and that the United States is not maintained in its position above sea-level by the rigidity of the earth, but it is, in the main, buoyed up, floats, because it is composed of material of deficient density, and (2) values of the equatorial and polar dimensions of the earth derived.

In 1910, Mr. Hayford published a continuation of this investigation, "Supplementary Investigations in 1909 of the Figure of the Earth and Isostasy" in which more observations were used than in the previous work. In this work he again derived values for the dimensions of the earth which were slightly different from those derived in the previous work. These values have been accepted by the scientific world as probably the most accurate which can be derived without an undue amount of further time and money spent in collecting more data and in the resulting computations.

Probably no work of any man within a century has attracted more attention from geodesists, geologists, and geophysicists, than has this work of Mr. Hayford's. It brought to him international fame, and has placed him in the front ranks of the world's great geodesists. R. S. Woodward, President of the Carnegie Institution of Washington, and an eminent scientist himself, in a letter to Mr. Hayford characterized the work as one of the most important contributions made to geodesy since the time of Gauss, and he predicted "that the verdict will be that you have succeeded in pushing knowledge one step beyond the point attained by Gauss, Bessel, Clarke, and all the rest."

Colonel Sir Sidney G. Burrard writing in 1925 says, "When we consider that the conclusions which he originally put forward in 1909 have never had to be modified, we have to acknowledge that the stamp of genius rests upon his work."

These and many other quotations from well-known geodesists and scientists all bear evidence of the high value placed on his work. But they all pay tribute to the courage of the man in attacking such a stupenduous piece of work.

Previous to Mr. Hayford's publication Colonel Burrard had the view that the geodetic results deduced from observations in the outer Himalayas and in the plains at their feet were not in accord with the theory of isostasy. Investigations made by him along the lines suggested in Mr. Hayford's treatise caused him to change his views. The very evidence which formerly seemed to be unfavorable to the theory of isostasy proved an unexpected support for it, and in a most able treatise issued in 1919, he

showed that the condition of isostasy exists also for India, and confirms the general deductions made by Mr. Hayford.

In general it is inevitable that opponents will arise to question the validity of any new revolutionary scientific theory. In this, Mr. Hayford's work on isostasy has been no exception.

One able scientist has criticized Mr. Hayford's work on isostasy and points out what he considers a rather serious defect in the work. His contention is that Mr. Hayford was in error by building his method around the untenable theory of complete local compensation, involving an unnecessarily complex mathematical treatment, and ignoring regional compensation. In his summary he states that "the isostatic compensation of the topographic features of the earth's crust must be regional to some extent. Perfect compensation is inconceivable; it could only be obtained if the material were so plastic that no surface irregularities would remain. The departures from complete local compensation are sufficient in their effect to require that they be considered in a complete reduction of gravity observations." In a letter to the writer under date of February 14, 1929, this scientist states, "The work on this subject which he planned was so impressive in magnitude and results, and is so really important, that the above has passed practically unnoted for twenty years."

In reviewing, however, the comments and discussions regarding his work, one is struck by the unanimity with which all accept it, and by the expressions of accord, indicating that Mr. Hayford's work is one of the outstanding pieces of research along geodetic, geophysical, and geologic lines.

In addition to bringing to him international fame through the award of the Victoria Medal, and by the adoption of the Hayford Spheroid, his theory of isostasy also brought to him local honors. His paper before the Western Society of Engineers at Chicago on "The Establishment of Isostasy," presented May 26, 1924, won for him the Chanute Medal of the Society as one of the three best papers read before the Society during the year 1924. The date of reading this paper was chosen by the Western Society in celebration of the occasion of the presentation of the Victoria Medal, which presentation was made on this same date

before the Royal Geographical Society of England and at which Mr. Hayford was not able to be in attendance. Formal presentation of the Chanute Medal was made on June 3, 1925, at the annual banquet of the Society. As Mr. Hayford's death had occurred the preceding March, the medal was received by his son, Maxwell Hayford.

International Spheroid of Reference

At the Constitutional Assembly of the International Geodetic and Geophysical Union (supplanting the former International Geodetic Association) held at Brussells in July, 1919, the question of an international ellipsoid of reference was brought to the attention of the delegates. This was brought up again at the First General Assembly of the Union held at Rome, May, 1922, when it was recommended that a spheroid should be fixed upon as early as possible so as to insure uniformity in geodetic work and computations. In accordance with this recommendation the section of Geodesy of the Union directed its Executive Committee "to fix, and, as early as possible, to recommend for cartographic calculations a spheroid of reference common to all countries on one continent." A serious study was made of this question by the executive committee, and reported upon to the Madrid Assembly of the Union held in October, 1924. After long discussion the proposals were ratified by the Section of Geodesy, at which some fifty members, representing twenty-five nations, were present. (1) Almost unanimously it was agreed to adopt an international ellipsoid common to all the continents; (2) almost unanimously it was agreed to adopt the flattening given by Hayford in his work, "Supplementary Investigations in 1909 of the Figure of the Earth and Isostasy." p. 77; (3) by the majority it was agreed to adopt the semi-axis given by Hayford in the same work. It was, therefore, recommended and adopted that the International Spheroid of Reference should be the one characterized by the following:

Semi-Major Axis 6,378,388 meters $\pm$ 18 meters
Flattening 1/297.0 $\pm$ 0.5

as given by Hayford in "Supplementary Investigations in 1909 of the Figure of the Earth and Isostasy."

It was recommended to scientific men for use in all scientific investigations where the shape and size of the earth were involved, and it was also recommended to those countries which, at the time, had not adopted a spheroid of reference for their triangulation, surveys, and maps, and to those countries which, having adopted one of the older spheroids, desired to make a change to a more accurate one. Hayford's Spheroid was chosen as the one which represented with the greatest precision all of the land surface of the earth. The values computed by him have about four times the weight of the others. These values, though deduced from observations made in the United States, have been shown to apply equally well to other regions.

This spheroid of reference is now generally known among scientists as the "Hayford Spheroid" and Americans may well be proud of this signal honor conferred upon one of its scientists.

It is interesting to note that General Georges Perrier, Secretary of the Section of Geodesy of the International Geodetic and Geophysical Union, has had tables computed and published for the Hayford International Spheroid. Furthermore, it is understood by the writer that Finland has already started computing its geodetic work on this spheroid.

When Dr. William Bowie, President of the Section of Geodesy and delegate to the Madrid Meeting, notified Professor Hayford of the adoption of the 1909 spheroid, he wrote, "This is a fine tribute to you."

In this as in many other cases Hayford's reply was very characteristic: "I am not unmindful of the fact that I was merely the leader of the team when this work was done. In this as in other cases, the leader gets a large part of the credit for work done by the team."

Intensity of Gravity and the Figure of the Earth

After making tests of the isostatic condition of the earth's crust and the figure of the earth by means of triangulation and astronomical observations, as already described in this memoir,

Professor Hayford decided that he was now in a position to make a further test by the use of values of the intensity of gravity, determined at many places in this country. This problem he had had in mind for some years and had laid plans for and carried out a campaign of gravity work in order to have data on hand, storing up the material gathered to be ready for use when he had finished his work on the deflections of the vertical.

The opinion was formed about 1900 that in order to secure the highest efficiency in the geodetic work of the United States, in the effort to increase the knowledge of the figure and size of the earth, it was advisable to postpone gravity determinations until the existing triangulation in the United States had been fully supplied with astronomical observations, and the resulting deflections of the vertical had been carefully studied. It would then be possible to utilize the information given by these observed deflections of the vertical in selecting such locations for the gravity stations that a given number of stations would furnish a maximum of valuable information. For it appeared that the deflections of the vertical would probably furnish valuable indications in regard to points in the United States at which the intensity of gravity might be expected to be normal, to be abnormally small, or to be abnormally great.

In accordance with this opinion few determinations of gravity were made during the years, 1900-1908. In 1907 Professor Hayford turned his attention seriously to a proposed gravity campaign. Methods of observing and computing were carefully gone over, and he suggested a number of changes. His studies led him to believe that control of temperature should be most carefully considered, as therein lay one of the greatest possible sources of error. Also he came to the conclusion that the static method of correcting for flexure of the pendulum case under the influence of the swinging pendulum could be improved upon. This latter conclusion led to the adoption of the interferometer method of measuring the flexure, an account of which is given elsewhere in this memoir.

On his recommendation the Superintendent of the United States Coast and Geodetic Survey approved Hayford's plan to have a number of additional gravity measures determined at selected points in this country. The writer, then in the field force of the Survey, began a gravity campaign in January, 1909, using the half-second pendulum, and continued on this work until he severed his connection with the Coast and Geodetic Survey in September, 1910. The gravity campaign was, however, continued for several years thereafter.

A new method of computation of the effect of topography and of isostatic compensation upon the intensity of gravity at a station was developed by Hayford and tested at a number of stations. A statement of the method and of the results of the test was given by him at the Sixteenth General Conference of the International Geodetic Association held at London and Cambridge, England, in September, 1909. On his return to the United States Mr. Hayford resigned from the Coast and Geodetic Survey to accept the position of Director of the College of Engineering at Northwestern University.

Dr. William Bowie was made Assistant Chief of the Computing Division and Assistant Inspector of Geodetic Work early in 1909, and served in the office as Hayford's understudy until the latter left the Bureau in October, 1909. During that year the two worked together on the application of isostasy to gravity determinations, and they continued their cooperation after Professor Hayford left the Survey. The result of their joint work was a report by the Coast and Geodetic Survey which appeared in 1912 and which was entitled, "Effect of Topography and Isostatic Compensation upon the Intensity of Gravity."

This investigation is based upon determinations of the intensity of gravity made at 105 stations, 16 selected stations being in foreign countries. In the principal computations full account is taken of the effect upon the vertical component of the attraction of gravity at a station, of all the topography of the world, and of the isostatic compensation of that topography assumed to be complete and uniformly distributed to the limiting depth of compensation, 113.7 kilometers, the value derived in his study of

deflections of the vertical. Use was made of the template method of computation, which materially speeded up the work. By this method a computer in an average of seventeen hours of work obtained the effect of all the topography of the world and its isostatic compensation upon the vertical component of the attraction of gravity at a given station.

For comparison purposes, the gravity anomalies for these same 105 stations were computed by the two methods of computation most generally accepted in recent years—the Bouguer method, and the free-air method. The "free-air" method ignores topography as if it had no effect whatever, or as if it were compensated for at zero depth. The Bouguer method takes into consideration the topography near the station, but ignores the distant topography and the curvature of the earth in computing the effect on the intensity of gravity. Hayford's methods are based on the assumption that the earth's crust is in a state of approximate equilibrium called isostasy. Besides the correction for topography to the theoretical value of gravity at any station there is also applied a correction of opposite sign to account for the effect of the compensation. The effect of the topography and its compensation for the whole earth is considered at each station.

The general result of this investigation was that isostatic compensation was found to be very nearly complete under all parts of the United States. Also that Hayford's method of reduction gave a closer approximation to the truth than either the free-air or the Bouguer method.

It may be stated that, as a result of the work of Professor Hayford while with the Coast and Geodetic Survey, it was proved that isostasy was probably a universal condition of the earth's surface. His conclusions in regard to isostasy were not, however, generally accepted by students of the earth, largely because it was a new subject and did not fit into generally accepted theories regarding structural and dynamic geology. The second reason was that Hayford had based his conclusions on rather scant evidence, that is, the triangulation used in the figure of the earth investigation formed a mere skeleton over the coun-

try, and that in the gravity investigation too few stations were involved. Investigations made since by officers of the Coast and Geodetic Survey in which more data were used, and the data involved were for stations in other countries in addition to those in the United States, have proved that isostasy is a scientific principle. There are few today who will deny the existence of isostasy for any considerable portion of the earth's crust.

The work of Dr. Vening Meinesz in determining the values of gravity at sea from observations on a submarine has proved that isostasy exists in ocean areas to as marked a degree as has been found true for land areas. The pioneer work of Hayford in attacking so vast a problem as the isostatic condition of the earth's crust, stands out as one of the notable accomplishments in the entire history of geodesy. Not only did his isostatic investigations lead to his election as a member of the National Academy of Sciences, but they brought to him the award of the Victoria Medal.

"For more than a century observers have been patiently determining the variation in the force and direction of gravity in all parts of the earth; observations are still being multiplied, new regions are being tested, the surface of the ocean and even its depths are being tested. What is the aim of this enthusiastic army of workers? It is to accumulate a world-wide series of results, until the rare man arises who can interpret their meaning. Hayford interpreted the gravity results; it was he who discovered their meaning.

"The foundations of Hayford's work were the 'observed facts'; he never allowed his imagination to lead him astray, he utilized mathematics as an instrument for dealing with his observed facts. When he had eventually discovered conditions in the earth's crust which had not been foreseen by either geologists or mathematicians, he presented his conclusions to the scientific world in the simple language of an engineer: 'Here are the observed facts, and here are the conditions of the crust that explain them.'

"The conditions that have arisen in the discussion of Hayford's Theory of Isostasy have not been due to his methods, but

to our inability to comprehend the conditions which have been shown to be existing. Conditions have been discovered of which we had had no experience at the earth's surface, and which were beyond the range of our imagination."—S. G. Burrard, in *The Geographical Journal of the Royal Geographical Society,* July, 1925, London.

Mechanism of Isostasy

In Hayford's work on isostasy he dealt mostly with the mathematical and geological sides, with but little consideration of the mechanics or physical interpretation of isostasy. This was a phase of the subject upon which he had spent considerable thought, but as shown by the following extract from a letter to Colonel Sir S. Burrard, September, 1920, he was not fully prepared or inclined to undertake researches regarding the physical explanation of isostasy:

"I have at various times studied a good deal on this particular part of the matter. I feel confident that, given sufficient time, I can show a good first approximation to the mechanism, a first approximation in accordance with a great mass of facts known to the physicist, chemist, engineer, geologist, and geodesist. I believe that the mechanism is simple in essence, but rather varied in the manner of its action under the great variety of conditions surrounding various cases in different parts of the earth and at different times. However, I have found such extreme difficulty in getting my views across into the minds of other men that I do not care to make any further attempt unless, and until, I can attempt it with such ample time at my disposal as to make the effort effective. I find that the greatest obstacle to progress is that most men persistently look at one factor in a problem at a time, whereas, especially in such a case as the one under consideration, one must keep a considerable number of factors in mind simultaneously to get a true understanding."

Interferometer Applied to Gravity

As noted elsewhere in this memoir, Mr. Hayford had assisted Professor Gore on pendulum observations for determining the

intensity of gravity, and in conjunction with Professor McNair had utilized the pendulums in similar work at the North Tamarack Mine, Calumet, Michigan, in 1902. Also, in his official capacity, he was very well informed regarding the apparatus and methods in use at the Survey for such work. One of the points which he considered weak was the method of determining the flexure of the pendulum case and its effect upon the resulting value of the intensity of gravity. The method employed was the static method of applying a known pull and from the flexure of the case compute what it would have been under the effect of the swinging pendulum, the correction to the period of the pendulums having been determined experimentally for various degrees of stability of the support of the case.

Professor Albert A. Michelson about this time had published his splendid monograph on "Light Waves and Their Uses," in which the Michelson interferometer and its use for measuring exceedingly small displacements was described. This was closely studied by Mr. Hayford and his keen mind soon grasped its possibilities in connection with gravity work.

In October, 1907, the writer was stationed at Barnegat Light House, N. J., engaged in making observations for primary azimuth, latitude, triangulation, and magnetics. Here he was joined on October 15 by Mr. Hayford, who as Inspector of Geodesy was making a field inspection trip to several of the field parties. He remained with the writer at Barnegat for five days watching the progress of the work at hand and assisting in observing and computing. Part of the time, however, he could be found under the lee of a neighboring hedge studying and computing and making notes. The night before leaving, he turned over to the writer the computations and notes upon which he had been working, together with a copy of Michelson's "Light Waves," with verbal instructions to go over the whole carefully before the field season ended, as the matter contained therein would probably be the writer's next assignment. Between this date and November 5, when the field season closed, the writer spent all of the time which could be spared from his other duties in studying the material handed over to him by Mr. Hayford.

These notes and computations proved to be a study on the possible use of the interferometer as a means of directly measuring the flexure of the pendulum case during the swinging of the actual pendulum, and showed mathematically the magnitude of the fringe displacement which could be expected in the interferometer, and other related but necessary points.

The problems confronting him were: (a) could the then existing instrument be so remodeled as to be used under the new conditions; (b) could such a remodeled instrument be used under the varying conditions incident upon field stations as to temperature, vibration, etc., without undue expense or loss of time to the field party; (c) could the correction to the period of the pendulum be obtained accurately in terms of the half-second shift of fringes in the interferometer due to the flexure of the pendulum case under the influence of the swing of the half-second pendulums used in gravity work.

The writer reported to the Washington office of the Survey and under instructions dated November 5, 1907, was assigned to the Division under Mr. Hayford. Preliminary experimental work was carried on at the Bureau of Standards, where a Michelson interferometer and the facilities of the Bureau were kindly placed at the disposal of the writer by Dr. S. W. Stratton. By December 10 the preliminary tests were carried far enough to satisfy questions (b) and (c) and the matter was laid before Mr. E. G. Fischer, Chief of the Instrument Division of the Survey, who solved question (a) by designing the modified form of interferometer now used for many years by the Survey.

In the fall of 1908, after returning from an extended field trip, during which time the modified forms of interferometer had been constructed, the writer was again assigned to the work with a view to perfecting the methods of, and computation incidental to, its use as an actual field instrument. This was successfully accomplished, with the result that the static method is now in practically all cases superseded by the interferometer or direct method.

This rather lengthy discussion of the interferometer is here given to show to what great length credit should be given to

Mr. Hayford. However much credit is given to the writer for his part in this work, since it was under his name the monograph on this subject was published by the Survey, it was Mr. Hayford's keen mind which conceived the idea; it was his power of abstract analysis which proved it theoretically possible; and it was his courage in backing the work which made it actually a success.

INTERNATIONAL GEODETIC ASSOCIATION

Dr. Hayford was twice honored by being sent to Europe as one of the two delegates selected to represent the United States at the General Conference Meetings of the International Geodetic Association, Mr. O. H. Tittmann, Superintendent of the United States Coast and Geodetic Survey, and Member of the Permanent Commission, being the other delegate.

The first of these meetings attended by Dr. Hayford was that held in 1906 at Budapest, and was the fifteenth General Conference of the International Geodetic Association. The report to the conference by the delegates from America is contained in a publication by the Department of Commerce and Labor and gives a summary of the geodetic operations in the United States, 1903-1906. In addition to a report upon the general operations of the Coast and Geodetic Survey, a second report was made by Hayford regarding his investigations of the figure of the earth based entirely upon observed deflections of the vertical in the United States. The first report of these studies was made by Mr. Tittmann to the fourteenth General Assembly of the Association held in Copenhagen in 1903, a work which reported upon later in 1909 was to bring to Mr. Hayford international fame. The meeting at Budapest is of special interest because it was the subject of his last talk to the engineering students at Northwestern University on December 19, 1924, when at the annual engineering banquet Professor Hayford gave a most interesting talk on the court reception at Budapest at which the delegates to the Geodetic Association were the honored guests.

Again in 1909, Tittmann and Hayford were the American delegates to the sixteenth General Conference of the International Geodetic Association held at Cambridge and London, at which a report was made upon the geodetic operations in the United States 1906-1909. At this meeting Hayford gave a further report of his investigations on the figure of the earth, and submitted his newly computed values for the dimensions of the spheroid, values which were later adopted by the Conference of 1924, assembled at Madrid, as the dimensions of the International Spheroid of Reference and now designated as the "Hayford Spheroid."

READINGS AND STUDIES

Previous to 1900, while in the Coast and Geodetic Survey, Mr. Hayford's readings and studies were extraordinary in scope of subjects and in number of publications read. He has left a list, on the Survey computing paper, of over six hundred such publications read, the latest recorded one being in 1900. This seems to have been the period when he was making himself conversant with the many lines of work and gathering information so necessary in his work. It must be remembered that this study and reading was mostly done at the time when a prodigious amount of labor was demanded of him in taking over the work of Mr. Charles A. Schott as Chief of the Computing Division and in carrying on the work of his newly created position as Inspector of Geodesy. The range of his studies during this time is best shown by the index to the one hundred and one pages of these studies:

Tidal theory, tidal observations and discussions, waves, seiches, earthquakes, physical hydrography, computing machines, mathematics, geodesy, least-squares, electricity and magnetism, astronomy, thermometry, density, weighing, meteorology pressure measurements, important numerical results, base measurements, variation of latitude, triangulation, leveling, station errors, and mean star places.

For each book or article read there is given the complete reference as to publication and author, and a brief summary of the

contents. To have kept this running account of his studies and readings was in itself a great task, and only the most methodically minded man could have done it. Evidently the burden became too great, for nothing similar to it is found in any other of his papers with the exception of one pocket-notebook dealing exclusively with geologic studies made, apparently, during the time he was working on the figure of the earth and isostasy.

While he kept no such list as above, he nevertheless did a vast amount of reading and study along engineering lines, in education, and in the physical sciences.

After taking up his work at Northwestern University his time was so fully occupied by his administrative duties, his teaching, and his researches, together with many outside activities, that there was left scant time for such a large range of reading. He kept up with the general current literature, however, to some extent through the medium of his class in "Journals." This class was composed of senior and fifth-year students and met one afternoon each week. It was conducted by having the students spend part of the time in the library making selections of articles in current engineering periodicals, then about three-quarters of an hour was given over to the study of these articles, after which the remainder of the afternoon period was devoted to reports by the students on their selected topics with discussions of the same. During the study period of this course, Professor Hayford usually devoted his own time to reading and study of selected articles in the current periodicals or other readings that he had on hand for the purpose. Each student was required to abstract his article on 3 x 5 cards and turn them over to the Director. Thus he accumulated a very great amount of information regarding the important engineering topics of the day. Practically no other reading was done by him during the school week, and as he usually had important engagements to take up his evenings, little time was granted him for reading, excepting at odd moments and during Sunday. How he managed to keep himself so well informed on the many subjects at his command is one of the marvels in connection with his program of life.

UNITED STATES COAST AND GEODETIC SURVEY DIRECTORSHIP

In 19— Mr. Hayford was approached by a man very high in the scientific life at Washington in regard to his becoming a candidate for Director of the United States Coast and Geodetic Survey. Hayford took this matter up with another man, also prominent in Washington, in order to get his view on the matter. Correspondence kept by Hayford indicates that these two men are the only ones having knowledge of the subject, but they were so high in scientific circles that there is little doubt but that their recommendations would have carried considerable weight in regard to the appointment. Concerning this Hayford wrote: "The position as head of the Coast and Geodetic Survey appeals so strongly to my professional ambition, because I have so great an admiration for the Survey and for its possible future, that if the appointment ever came to me at an adequate salary, a security of tenure for at least ten years, and a security against political interference with my administration as Director, I should expect to accept it and go into the work with a vim.

"I should feel in duty bound to accept on the above basis . . . especially as I have had an experience which qualifies me especially for the position. The Coast and Geodetic Survey has an excellent past record. I believe the future possibilities are great for rendering efficient service to the people of the United States in making good surveys, in producing excellent charts, and in making valuable fundamental scientific work.

"I see definite attainable possibilities of decided improvement in the geodetic work, in its hydrographic work, in surveys of the coast from airplane, in new instruments, in its tidal work, in the precise leveling, in the character of its charts, and in the way its charts are used to promote safety in navigation.

"I believe I have the proper background of training and experience to enable me to succeed under favorable circumstances in making these possible improvements a reality. For all these reasons I would enter the position of Director of the United States Coast and Geodetic Survey with great enthusiasm, *pro-*

vided I entered it under conditions favorable to success, i. e., adequate salary, freedom from fear of short tenure, and freedom from fear of political interference."

Evidently these conditions were not possible in their fulfilment, for the name of Hayford does not seem to have become publicly listed for the position, or at least Mr. Hayford thought they could not be met. In setting forth the advantages of this position one of the two scientists wrote him: "It is a great institution, with a record of achievement of which one may well be proud. It would be an honor and a privilege to guide its destinies to still greater achievements. I am satisfied that you can do this better than any other man. You have the background from long years of intimate contact with the work, worldwide recognition as an authority in geodesy, executive experience, and vision, energy, and enthusiasm. Where else can these attributes be found? I say these things judicially, untempered by my admiration and affection for my friend."

For obvious reasons the writer has omitted dates and the names of the two who were interested in this matter.

In the foregoing the endeavor has been made to give a report of the contributions which Hayford made to geodesy. Most of these contributions were made while he was a member of the Coast and Geodetic Survey, hence, in a way, they also tell of the progress which the Survey made while he was at the head of the geodetic work and of the computing division. These divisions and Hayford are so closely related that what may be said of the one should be said of the other. The glory of the one becomes the glory of the other. While the work of the Survey before Hayford was of a very high degree of excellence and worthy of much praise, under his leadership the strides that were made in geodesy attracted the attention of the world. It is generally recognized today throughout the scientific world that Professor Hayford laid the foundation for the great progress that the Coast and Geodetic Survey has made in both its scientific and engineering work and in the field of isostasy during the past thirty years. The Survey is universally recognized as the

leading geodetic organization in the world not only in its practical work but in the interpretation of the geodetic data collected. This advance has since been ably carried on under the leadership of his successor, Dr. William Bowie, until the United States in its geodetic work stands, probably, unrivalled today. A test of the greatness of the geodetic work of the Survey may be had in a review of the comments of prominent men in other organizations and countries, and by men who are qualified to judge. They all accord to the Survey a very high place in the geodesy of the world. Commandant Perrier, the French Geodesist, says:

"There is no example in the history of geodesy of a comparable collection of measurements, made with so much determination, such rapidity, and such powerful means of action, and guided by such an exact comprehension of the end to be attained."

While living in northwest Washington, Hayford's home was too far away to permit of his taking noon luncheon with his family at home. Consequently he took his lunch at places near the office of the Survey, usually in the company of one or more congenial friends. Noteworthy among luncheon groups was the one which usually left the Survey at noon to partake of the midday meal at the Library of Congress Cafe. This group usually consisted of Hayford, R. L. Faris, Assistant, and now Assistant Director of the Survey, George R. Putnam, Assistant of the Survey, and later First Commissioner of Lighthouses, William Bowie, Assistant, and now Chief of the Division of Geodesy. It was a merry, congenial group and many were the scientific problems discussed during the noon period.

RESIGNATION FROM THE U. S. COAST AND GEODETIC SURVEY

In 1908 the Trustees of Northwestern University extended an invitation to Mr. Hayford to become the head of the newly established College of Engineering at Evanston, Illinois. This offer

was accepted and he was officially elected Director of the College in October, 1908, with the understanding that he would take up his duties at Northwestern in September, 1909. Owing to the fact that he was detained in his position at Washington and sent as one of the delegates to represent the United States at the Conference of the International Geodetic Association held at London and Cambridge, England, September 21-29, his formal resignation from the U. S. Coast and Geodetic Survey was not handed to O. H. Tittmann, Superintendent of the Survey, until November 12, 1909. In tendering his resignation he wrote:

"I thank you for the strong support and hearty sympathy which you have uniformly given me in my work in the Coast and Geodetic Survey. I leave the Survey with regret and only because I am convinced that as Director of the College of Engineering at Northwestern University, which position I have accepted, I have a greater opportunity for usefulness than it is possible for you to give me in the Survey."

In accepting Mr. Hayford's resignation to take effect at the close of business on November 23, 1909, Superintendent Tittmann wrote:

"I fully share the expression of regret with which the Secretary has accepted your resignation, for no one is more fully conscious of the loss which the Survey has sustained than I am.

"The signal ability with which you have served the government, your industry and loyalty, have established a standard which will always be pleasant to refer to and to look back upon."

In reference to Mr. Hayford's resignation from the Survey, Mr. Charles Nagel, Secretary of the Department of Commerce and Labor, expressed himself as follows:

"The terms of commendation with which the Superintendent of the Coast and Geodetic Survey has transmitted to me your letter of resignation, make it a pleasant duty to take more than passing notice of your resignation, which I accept with deep regret, because your going is a distinct loss to the government. On the other hand, the evidence of the unusual ability, the zeal and the wisdom with which you have served the government for a long period, warrant the assumption that a multitude of stu-

dents, and through them the country at large, will benefit by the same qualities which have distinguished your service to the Government."

THE COLLEGE OF ENGINEERING AT NORTHWESTERN UNIVERSITY

Mr. Hayford was elected Director of the College of Engineering at Northwestern University in October, 1908, to take up his duties in September, 1909. He entered into this work with enthusiasm because he felt that here was a field for unlimited service to the engineering and scientific world. He had long held strong views as to the kind of training an engineer should have and in the type of curriculum to be adopted at Northwestern he had vision of the fruition of his views.

The motto of the College of Engineering of Northwestern is "Culture for Usefulness." In order to realize to the fullest extent the import of the motto it was decided that the usual four-year course was too short a time in which to give the necessary technical training for engineers, and at the same time to broaden his education by the inclusion of many of the more cultural subjects. Hence, the adoption of a five-year program leading to the professional degree. It called for an effort to obtain for the student the broadest culture attainable in five years in order to equip him, as a man and an engineer, for the greatest possible usefulness in the world in the united struggle of man for progress. It was the aim to train future engineers for the greatest average effectiveness in a lifetime rather than for the greatest effectiveness in the first years after graduation.

Only two curricula were offered—one in Civil Engineering, and one in a combined Mechanical-Electrical Engineering course. Up to the time of Director Hayford's death only slight modifications were made in these curricula. The engineering training in the narrow sense commenced in the freshman year and continued throughout each of the five years, occupying a large portion of the curriculum in the last two years, and a relatively small proportion in the first three. The general education, represented by the courses normally given in the College of Liberal

Arts, extended throughout all of the five years. This arrangement was based upon the idea that a liberal arts education is an essentially inseparable part of an engineering education—not a matter separate and apart, to be secured first; that it is in part essential to fit the engineering student into the world's work, to enable him to use his technical knowledge effectively, and to live a richer and more contented life.

While the engineering students were under the continuous control of the engineering faculty during the five years, they merged with the liberal arts students in classrooms and in campus, social, and athletic activities. It was believed that this control from the start by the engineering faculty, combined with the close contact with the liberal arts faculty and with students having other ambitions than his own, would give the engineering student early orientation and unusual breadth of view.

In his appointment at Northwestern University, Director Hayford held the title of Director of the College of Engineering and Professor of Civil Engineering. As professor he was supposed to do a small amount of teaching. This teaching consisted of two courses, "Public Relations of Engineers" and "Journals," both courses being required for all students before graduation and taken in their senior or fifth year.

These two courses were virtually of his own invention and showed him as a man keenly alive to what kind of a training an engineer should have. His belief was that the purely technical side of engineering was not sufficient for the best type of engineer and that the student should early learn about the human side of engineering through contact with men, through a study of men as made evident by their success or failure, and in a study of them and their works through their writings.

In the course of Public Relations he was well fitted to guide the young engineer for he was closely in touch with many of the great men of the scientific world; he knew a great many of them very intimately and many more through their writings. He was thus able at all times to supply the needed factors to make clear to the class why success or failure had come to the person

in question. His fund of information along these lines was most astounding.

As given in his initial lecture to this class, the purpose was to study the relations of human beings with reference to success, of the best kind of success; true success that is worthwhile as measured by accomplishment, by the degree to which a task is accomplished. Biographies of the more successful men were studied and analyzed, the ethics of the profession taken into consideration, how to work in organizations and with administrations and officials.

His varied life as student, computer, teacher, field observer, and administrator and educator made him an ideal teacher; to the classroom he brought a knowledge of the sciences and of contact with scientists, engineers, and educators given to but few men. His analytical thought, breadth and depth of knowledge made him a most wonderful teacher. He not only possessed the requisite knowledge, but he took great interest in his teaching and had that most important ability of being able to impart the knowledge he possessed. His students respected him not only as an instructor, but because of his sterling character. The more mature his students, the more mature they were in their praise.

Director Hayford thoroughly believed in having his faculty keep each other informed regarding their work, especially along the lines of research and studies outside the classroom, and also that they themselves keep in touch with engineering and the scientific world in general. Partly to meet this need he organized the Faculty Colloquium, which met normally every two weeks and at which each faculty member, acting in rotation, was responsible for the hour devoted to the meeting. A member at his own discretion could talk regarding any research problem he was engaged upon, or on matters relating to the college, or review some question of importance in the scientific or educational world. The dates for the entire year were assigned for each meeting and speaker, the assignments extending throughout the scholastic year.

His contact with the student body was a close and intimate one, especially in the earlier years of the régime at Northwestern, and before the students had increased to a number too large to permit of this close intimacy. When he assumed the position of Director he constituted himself the student adviser for all students, and regularly twice each year, every student was called to his office for conferences in regard to his work in college; programs of studies for the coming year were studied, and at registration he personally supervised the schedule of course for practically every student registering. His office hours were from 9 A.M. to nearly 5 P.M., and his door was always open to any student who wished to confer with him. Never did the writer know of a case where a student was forced to apply to a secretary or clerk before being admitted to the Director. Practically at all times the students had free access to Director Hayford; this, of course, led to serious interruption, but he did not seem to mind it very much. He would simply make a notation or mark regarding his work, turn to the student and pay strict attention. When the conference was ended he would take up his line of thought or work again as though no interruption had occurred. If the matter was important enough he would carefully write out a detailed statement regarding the conference and file for reference. He trusted little to his memory.

Usually his office was the working shop for the computers who helped him on his researches. In this way he was always close at hand to take account and direct their work with a minimum loss of time to himself.

In his relations with his faculty he was invariably fair, just, consistent, and considerate of all interests and seldom exercised with authority his official position. The only thing which he seemed to hold in contempt was bad work in pupil or professor. He did not apparently care for display of any kind, and it seemed that no desire of approbation influenced his actions, though he always seemed pleased to inform his intimate friends of any honors that came to him.

His standards of work and accomplishment were high. His influence as Director, and his commanding position as a scholar

and a scientist, were naturally great and were always exerted in the direction of progress in the education of the engineering student.

He was an earnest worker on the faculty, especially in the furtherance of a rigid and broad scientific training and development of the mind. His motto seemed to be "train to think" and thus produce leaders in thought—in order to obtain the greatest development for later life rather than for the years immediately following graduation.

His long experience as a scientific man and scientific educator in this country, together with his wide scientific knowledge, gave him a very clear perception of a school of engineering which should train men capable of meeting the professional calls of the future. His ideals were that there should be no specializing until the broad fields of study in engineering and the humanities had been covered.

In his own college life he apparently laid the solid foundation of success, and wished to carry such a foundation onward.

Sincere he was, and truthful to the point of being unable to allow the shadow of deceit in his ways. In reporting on conditions within the College he always gave the *facts,* whether to his disadvantage as Director or otherwise. As one of his colleagues once remarked: "Hayford was so upright, he leaned over backward." This sometimes worked to his disadvantage, for it sometimes furnished opponents with ammunition to criticize his régime. Perhaps, sometimes it would have been more to his advantage had he glossed over some of the facts.

It was noticeable in his Directorship, as in the work at the Coast and Geodetic Survey, that more than one subordinate felt the cheer of his sympathetic appreciation.

As head of the College of Engineering his aid was much sought for as an adviser in matters involving scientific questions along engineering lines, especially those relating to his chosen fields of geodesy or geologic matters, and he was called upon several times to give expert court testimony in questions where solution demanded technical skill and knowledge. He had a fine grasp upon the true use of the calculus method of approaching

a problem, and his methods of approximate solutions of an intricate mathematical problem astounded all with whom he worked. President McNair of the Michigan College of Mines, himself a mathematician and scientist of no mean ability who had worked with Hayford in solving many intricate problems, once remarked to the writer that in his opinion Hayford had no equal in the world in handling mathematical problems, for when the ordinary calculus solutions failed Hayford's methods of approximation usually found a way.

The tasks of his position in the College, the preparation of scientific papers and lectures, the researches and computations in his favorite fields of geodesy, geology, evaporation and stream flow, and his large correspondence with scientific men and educators—these filled his time and seemed to employ all his time. No doubt he realized that his time was too fully taken up and that he would have liked to have more leisure for pure research. In a spirit far from boasting, but stating it merely as a matter of fact, he once remarked to the writer that if he could be sure of uninterrupted time for one hour a day, he could make a name for himself once a year.

The problems which he had in mind and upon which he would have liked to carry on research were many, especially in the later years of his life when stream-flow and evaporation were his important investigations. But his life proved all too short and only notes are left to show the lines along which he expected to progress.

Hospitality was a keen delight to him and to his family and his home was ever a place of entertainment to visitors from out of town or intimate personal friends. It was always a delight to visit them in their home. An atmosphere of good cheer was always present.

In conversation Director Hayford was most apt, because of a remarkable range of information on general and scientific topics, and with anecdote ready for the entertainment of guest or associate. He was ever cheerful and ever inclined to look upon the bright side of life, hopeful and sanguine of success where others might be discouraged. His keen sense of humor often carried

him over disagreeable obstacles, but he had a tremendous spirit when roused to just anger. Usually he had great control over himself in this respect and seldom did he allow anger to overcome him.

He was no recluse, but was most human in his associations with mankind. When traveling he never failed to associate himself with some man, whether of high or low degree, and in this way he put in practice his motto "Study Men." In a way he was not fond of "society," or social distinction, but did not shun it and usually was the life of a party at which he was present.

In his moral life Professor Hayford was almost without a peer. The writer has had a most varied experience with men in many walks of life, traveling on trains and boats, in camp life on surveys, in college life as student and professor, and in his association with men he can truthfully say that Hayford is one of the exceedingly few men of his intimate acquaintance whom he has never heard give voice to a "shady" joke or tainted story. Whether this was due to strict inhibition or to natural cleanliness, the writer cannot say, but it always appeared as though such thoughts were not a part of him.

As in his moral life, so was Hayford in regard to minor vices and appetites usually found in men. The use of tobacco and liquor was not a part of his make-up. While he did not use them himself, he was seldom heard to preach against them, and evidently they detracted little from his estimate of men with whom he associated. However, he was very severe in his censure of delinquent students who appeared before him with cigarette-stained fingers, and they usually received sharp reprimands on this account.

He was not a "gossip," unless it was something good or worth-while which he could speak of regarding another person. In this he was very careful. Dean Marston, referred to in another part of this memoir, wrote in 1929, "I find it impossible to tell why or just how Director Hayford made such a strong personal impression on me, or why we formed such an endearing friendship, continuing until the day of his death. I do not re-

call, at this time, a single mean thing he ever did, yet he was no sissy." His moral and mental cleanliness were noticeable to all his intimate friends and associates, and were a source of generous comment when he became the subject of discussion.

COSTA RICA-PANAMA BOUNDARY COMMISSION

The boundary between the Republics of Costa Rica and Panama in Central America had been a matter of dispute for many years. It was the subject of an arbitration in 1900 by President Loubet of France, but the award was not accepted by the two countries. In 1910 a new treaty between the countries was negotiated which provided for a new arbitration of the boundary dispute. Under this plan, the arbitrator was Chief Justice White of the United States Supreme Court. A boundary commission of engineers was provided for, of which each of the two countries would have one representative, and Chief Justice White would appoint two impartial American representatives.

The Chief Justice appointed as his representatives Director John F. Hayford of Northwestern University College of Engineering and Professor Ora M. Leland of the College of Civil Engineering, Cornell University, Ithaca, New York. Frank W. Hodgdon of Boston, Massachusetts, was appointed by Panama, and Percy H. Ashmead of New York was selected by Costa Rica as their respective representatives. These appointments were made in the fall of 1911. Director Hayford in his letter of acceptance as commissioner made the following conditions: that he should "not be required to leave Evanston before September 30, nor to be continuously absent from Evanston for more than twelve weeks at any one time" to "protect the University against the danger that this service might interfere, improperly, with the performance of my duties as Director of the College of Engineering."

The Commission was organized October 6, 1911, by electing Director Hayford as its Chairman and Professor Leland as its Secretary. It organized a large body of engineers and operating staff and sailed from New York on January 13, 1912, for

Panama, and arrived on January 24 at Bocas del Toro near the disputed territory, and the same day the party with all equipment reached the houses on the Sanchez farm, which had already been prepared for use as the field headquarters, on the bank of the Sixaola River near the end of the railroad. The survey parties left headquarters to commence field work on January 25-29. Surveys and explorations of the region involved in the boundary dispute were carried on under the direction of the Commission for the greater part of 1912 when the force was returned to the United States and disbanded. A tabulated statement in the report of the Commissioners shows that Director Hayford was present in the region of the field work from January 24 to March 19, May 14 to July 29, visiting the engineering parties and studying the topography of the country.

The Commission held twenty-six meetings from October 6, 1911, to February 19, 1912, and eight meetings July 22 to 27, 1912. Between February 19 and July 22, 1912, though no meetings were held the Commission was virtually in session continuously, for the members kept in touch with each other by mail or by cable. In all forty-one meetings were held, including those after completion of the field operations. Throughout the period Director Hayford adhered strictly to the "twelve weeks" condition of his acceptance, and this necessitated his doing considerable traveling to be at the various meetings, and to and from Panama.

The conditions under which the field work was done were very difficult, in a region where rainfall is heavy even during the dry season, in part through tropical jungle where cutting was practically necessary at every step, and in part where all supplies had to be carried on men's backs. Especially difficult was it to obtain astronomical observations on account of the dense growth of trees.

Director Hayford's portion of the work was the duties which fell to him as Chairman of the Commission, and during a part of the time as the only Commissioner present in the region of the field work. The writer has had the privilege of inspecting his personal diaries which he kept on all of the work and they

proved intensely interesting reading, kept as they were with his usual infinite detail.

The survey parties were disbanded gradually in San Jose after their return from the field work, the last one leaving for New York on November 20.

The preparation of the final Report of the Commission, including its collection of maps and photographs, was made at Evanston, Illinois, between October 25, 1912, and July 5, 1913, under the direction of Mr. Hayford, assisted by two transitmen of the field force and the disbursing officer who acted as stenographer. Four meetings of the full Commission were held in Evanston. The final report was submitted to the Arbitrator on July 14, 1913, and the work of the Commission was brought to a close.

Formal notice of the termination of the services of the Commissioner was sent under date of November 22, 1913, in which Chief Justice White stated: "And I say with perfect candor that the zeal, the character, and the high professional qualities displayed by the Commission have won my profound appreciation of their great merit, and entitle them to the gratitude of both governments who are parties to the treaty."

The opinion and decision of Edward Douglass White, Chief Justice of the United States, acting in the capacity of arbitrator in the boundary dispute between the Republics of Costa Rica and Panama, provided for by the Convention between these two governments under date of March 17, 1910, was formally rendered on September 12, 1914, and the work of actually marking and delineating the boundary was authorized September, 1921.

EVAPORATION FROM THE GREAT LAKES, STREAM-FLOW AND RELATED PROBLEMS

In the fall of 1909, when Mr. Hayford moved from Washington to Evanston, Illinois, to become Director of the College of Engineering at Northwestern University, he established his family in a delightful home on Ingleside Place, only one house

removed from the bluff overlooking beautiful Lake Michigan, and about one mile from the College. It was his usual practice, excepting in the most inclement weather, to walk to and from the College along the bluff or lake shore rather than use the walks. His active mind soon realized that before him, in the form of Lake Michigan and the rest of the Great Lakes, there existed a wealth of scientific problems of astounding interest. The first of these to receive more detailed notice was the problem of the seiches occurring in the lake, and often he would be engaged in studying them by actually measuring their magnitude and times of occurrence. He coupled this with a wide range of the study of the literature on the seiches.

Soon a much larger problem began to take form in his mind, and by the summer of 1911 had taken rather concrete form. He saw at his very door a most gigantic "Evaporation Pan," and visioned many of the intensely interesting scientific studies which lay therein. The first of these which he attacked was the problem of evaporation and of the wind and barometric effects upon the level of the Great Lakes, which soon broadened out to larger problems, the ultimate object of this larger investigation being to obtain a better formulation of the laws governing evaporation, rainfall and run-off, and in deriving therefrom better formulas governing the amount of stream flow. These problems continued to enlist his attention for the remainder of his life and, with the exception of his work as Director of the College, occupied the greater part of his time. The work was of such magnitude that he found he could not handle the mass of computing necessary and so placed the matter before the Carnegie Institution of Washington. They designated him a Research Associate of the Institution and awarded him grants of money to enable him to hire computers on the work.

These computers were for the most part advanced students in the College of Engineering, and they worked continuously during the various school vacations and at various periods during the school year. One student, Julius A. Folse, worked on the computations almost from their beginning and attained such proficiency that after graduation he accepted work as chief as-

sistant to Professor Hayford in carrying on these investigations, and to him was awarded the work of completing the second volume, as is shown later on in these memoirs. The help of these computers was especially needed in the long, involved least-squares solutions incident to the work. The first direct result of the work was given to the world in the volume, "Effect of Winds and of Barometric Pressures on the Great Lakes," published in 1922 by the Carnegie Institution as Publication No. 317.

This was followed by a second publication which Professor Hayford was not able to complete before his death, but upon which he was working when stricken in December, 1924. The work was, however, brought to such a state that it was completed in 1929 by Julius A. Folse and was published by the Institution as Publication No. 400. This second volume deals with "A New Method of Estimating Stream Flow, Based upon a New Evaporation Formula."

These two volumes, together with his work on Isostasy, are tangible evidence of the wonderful brain which could conceive a method of attacking such difficult problems, and the almost unbelievable courage of the man in facing the gigantic task of trying to work through such a mass of intricate and difficult computations. Only a man grounded thoroughly in the theory of the method of least squares, with a concise grasp upon the interpretation of results obtained, and with much practice in the solution of indefinite problems by this method would have the temerity in attacking them. In this kind of scientific research Professor Hayford probably had no equal. In his hands least squares was a most wonderful tool.

In the investigations of "Effect of Winds and Barometric Pressures," 74 complete least-squares solutions and the corresponding studies were made. In one of these solutions each observation equation contained 40 unknowns, and there were 619 observation equations in the set. More than 22,500 man-hours of time were spent on the routine part of the computations and studies connected with the broad investigation of evaporation, and of wind and barometric effects.

This investigation considers the lake-chain Michigan-Huron and Erie as a great "evaporation pan." The principal data used include hourly and daily observed elevations of the surfaces of Erie and Michigan-Huron at five gage stations, observed hourly wind directions and velocities at five points near these lakes, and the observed barometric pressures twice a day at six points, for periods from 1909 to 1913 as observed by the U. S. Weather Bureau, the U. S. Lake Survey, and the Corps of Engineers of the U. S. Army.

A summary of the results obtained in this research includes:

1. Reasonably accurate numerical expressions for the effects of barometric pressures at five stations.

2. General method developed for finding numerical expression at any station on any body of water from observations of the water elevation and the forecast maps of the Weather Bureau.

3. General expression, including numerical constants, for effect of winds, of any given velocity and direction, affecting elevation of the water surface at any given station, on any body of water, anywhere in the world.

4. The relation between seiches and the uncertainties in daily mean elevations at gage stations discerned.

5. Accuracy with which the elevations of mean surfaces of any one of the Great Lakes may be determined for any given day decidely increased.

The possible applications of the results of the investigations as summarized by Professor Hayford are:

a. Application to a study of the laws of Evaporation.
b. Application to regulation of the Great Lakes.
c. Application to determination of mean sea-level and to precise leveling
d. Application to determination of tilting of the Great Lakes region.

The second volume on the study of evaporation concerns itself with "A New Method of Estimating Stream-Flow, Based upon a New Evaporation Formula" as previously mentioned.

In it Professor Hayford (and as completed by Mr. Folse)* makes use of the evaporation formulæ derived in the first part and applies them to two specific streams for which stream-flow and meterological data were available. These two streams were in Wagon Wheel Gap, Colorado, and data were available through the kind permission of the Weather Bureau and the Forest Service.

A general summary of the outcome of the investigation shows:

1. Approximate quantitative expressions of the fundamental law of flow of the two streams in Wagon Wheel Gap.

2. Development of general method by which such expressions can be derived for any streams anywhere in the eastern two-thirds of the United States where annual rainfall is twenty inches or more and data of stream-flow and meterological observations are available.

3. The relation of the above to:

   a. Problem of increasing length of record of flow of a stream, the hydrograph, as a basis for greater accuracy in the design of works for power, irrigation, sanitation, and navigation.

   b. Problem of forecasting flow of a stream as a basis for increasing the economy of hydro-electric power plants.

   c. The problem of determining effects of forest cover on the run-off from a watershed.

While he was at work on the larger investigation of stream-flow and evaporation, he had a number of related problems in mind, and some of these were investigated as engineering theses by advanced students in the Fifth Year of the College of Engineering, working under the direction of Professor Hayford. It is worth while to consider here a few of these because they had a direct bearing on his proposed line of future work and studies.

* Mr. Folse states in the preface to this second volume that he resumed work on this study "primarily with the object of rounding out in form for publication the work already accomplished by Dr. Hayford."

Having developed the theories and formulas for evaporation and stream-flow, it was necessary to justify the expenditures of time and money in showing that they could be applied successfully in general practice in hydraulic work.

In 1924, Alva B. Simons, under the direction of Professor Hayford, made a study of a "Proposed Control of the Elevations of Lake Huron-Michigan and Erie by a Dam with Moveable Parts at the Head of the Niagara River." On the supposition that a desirable elevation to be maintained in Michigan-Huron as one-half foot above its mean annual stage, and similarly for Erie, the investigation showed that the proposed method of regulation would practically destroy the loss on Michigan-Huron of the 10,000 c. f. s. withdrawn by the Sanitary District of Chicago. It would entirely destroy the losses on Lakes St. Clair and Erie, due to the diversion, and in addition would appreciably raise the levels of these lakes.

In 1924, T. B. Stitt, working under Professor Hayford's supervision, began a study of "Probability of Floods in Streams in Humid Climates." In this work the investigation applied the laws obtained by Professor Hayford in his stream-flow, and through these found the relation of the actual discharge-frequency curves of each of four streams selected to their theoretical probability curves. On account of the sickness and death of Professor Hayford this investigation was not brought to a complete conclusion. Note is here made of it because this field of work, namely, prophesying stream-flow, was the field in which Professor Hayford was planning to do much work. He often discussed with the writer concerning the practical use of the proposed methods in their application to hydro-electric plant installation and operation. He had already applied his formulas for stream-flow to the streams in Wagon Wheel Gap with much success and was planning a further application to the Cumberland and Delaware Rivers.

In the fall before his death the writer made a trip with him to the hydro-electric plant of the Mississippi River Power Commission at Keokuk, Iowa. While there Professor Hayford made tentative arrangements with the management to obtain their rec-

ords of stream-flow and hydrological data on the Mississippi with a view to testing the accuracy of his laws on one of the largest projects in this country. His untimely death, however, brought an end to this work.

In 1924 Professor Hayford, J. A. Folse, and B. J. Fisher collected data on the Kankakee River, Illinois, and Mr. Fisher, under the supervision of Professor Hayford worked on the problem of estimating stream-flow on this river, and its application in the hydro-electric field. The results indicated very clearly the value of the method in prophesying stream-flow.

His publications and lectures soon brought him general recognition as an authority on the problems affecting the waters of the Great Lakes and his services were increasingly solicited in connection with these problems.

In the case of the United States versus the Sanitary District of Chicago, which was argued before Judge Landis in 1923, in which the United States sought to restrain Chicago from taking as much as 10,000 cubic feet per second of water from Lake Michigan, Professor Hayford was called in by the United States as an expert to establish the degree of accuracy and reliability of the gaging of the Niagara River. This was the crucial point in establishing the fact that there had been a lowering of Lake Erie, Lake Huron, and Lake Michigan, by the diversion of water through the Chicago drainage canal, below the levels which these lakes would have had under natural conditions.

Not only was Director Hayford keenly interested in the problems concerning the Great Lakes, but he also spent much time and study on the related problem of the St. Lawrence Waterway, in the value of which he was a firm believer as is made evident by many letters which he wrote regarding it. In one of these, addressed to Hon. C. R. Chindblom, House of Representatives, Washington, D. C., he writes, "I hope that the St. Lawrence Waterway will be built, the power along that river gradually developed, and certain other relatively minor improvements made for the benefit of navigation on the Great Lakes. Such action would bring large returns to the people of the United States by lowering the cost of transportation between the great

middle west and the seaboard and Europe, and by helping to relieve the inevitable congestion on the railways which is bound to occur occasionally for a quarter of a century."

When he was invited November 24, 1923, to become a member of the Committee of the Western Society of Engineers on the St. Lawrence Waterway he accepted with pleasure for he believed "such improvements to be very important to the people of both the United States and Canada, because it will bring vast returns in comparison with what it will cost."

On January 29, 1924, he was appointed and accepted membership on the Rivers and Harbors Committee of the Chicago Association of Commerce, and on many occasions was invited to address various meetings on topics relating to the Lake Diversion and Waterways. One of these addresses, "The Best Use of the Waters of the Great Lakes," was given before various groups in and around Chicago and was productive of much comment.

As a humorous side-light on his researches and addresses on the subject of the diversion of waters from the Great Lakes and his other activities, the honorary degree of S. G. (Steam Generator) was conferred upon Director Hayford at the December 13, 1923, meeting of the Chicago Club of Northwestern Men. The degree was appropriately conferred with the following tribute:

"John Accelerator Hayford"

"Beloved of all the poker players in the Cosmos Club of Washington, D. C.; discoverer of the cut-throats who are stealing waters from Lake Michigan, thus preventing navigation of schooners across the bar; a professor of engineering who knows more about aeroplanes than Dean Kendall knows about microbes; a consistent rooter at foot-ball games, win or lose."

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS

In the law establishing the National Advisory Committee for Aeronautics it is stated that "The President is authorized to appoint not to exceed twelve members, to consist of two mem-

bers from the War Department, from the office in charge of Military Aeronautics; two members from the Navy Department, from the office in charge of Naval Aeronautics; a representative each of the Smithsonian Institution, of the Weather Bureau, and of the United States Bureau of Standards; together with not more than five additional persons who shall be acquainted with the needs of aeronautical science, either civil or military, or skilled in aeronautical engineering or its allied science." President Woodrow Wilson appointed Professor Hayford a member as one of the "five additional persons" on April 2, 1915, when the Advisory Committee was first formed, and he served continuously in that capacity until his voluntary resignation in 1923. This duty caused him often to be absent from Evanston to attend the various meetings of the Committee held at Washington or at the several flying fields where tests of airplanes or airplane equipment were in progress. He took keen interest in this work and so far as the writer is able to judge from his correspondence he was never absent from any of these meetings. Judging from several passages in his correspondence, these meetings often occurred at the same time as meetings of other organizations in Washington and vicinity in which he was interested, and thus only one trip from Evanston was made necessary. Perhaps these coincident meetings were graciously arranged to favor Professor Hayford in this respect.

Personnel of National Advisory Committee,
April 20, 1922.

Dr. Charles D. Walcott, Secretary, Smithsonian Institution, Chairman.
Dr. Joseph S. Ames, Johns Hopkins University, Chairman, Executive Committee.
Dr. S. W. Stratton, Secretary, Director, Bureau of Standards.
Major T. H. Bane, Chief, Engineering Division, Army Air Service.
Dr. W. F. Durand, Professor of Mechanical Engineering, Stanford University.
Dr. John F. Hayford, Northwestern University.
Professor Charles F. Marvin, Chief, United States Weather Bureau.

Rear Admiral Wm. A. Moffett, Chief, Bureau of Aeronautics, Navy Department.
Major General Mason M. Patrick, Chief of Army Air Service.
Dr. M. I. Pupin, Columbia University.
Rear Admiral D. W. Taylor, Chief Constructor, U. S. N.
Orville Wright, Dayton, Ohio.

When Dr. S. W. Stratton resigned from the position of Director of the Bureau of Standards to become President of the Massachusetts Institute of Technology, and his successor, Dr. G. K. Burgess, had been appointed, it was apparent that Dr. Burgess should become the representative of the Bureau of Standards on the National Advisory Committee for Aeronautics, and unless Dr. Stratton could be appointed from private life he would necessarily have to be dropped from the Committee of which he had been the Secretary. There now occurred one of the finest examples of self-effacement which could possibly be conceived when, on May 21, 1923, Professor Hayford tendered his resignation from the Committee to take effect at whatever date the President should designate, in order to provide an opportunity for Dr. Stratton to continue on the Committee. In his letter of resignation he states:

"It is very important to keep Dr. Stratton on the National Advisory Committee for Aeronautics. . . . He has a continuing special interest in aeronautics on account of the advanced courses in that line at the Massachusetts Institute of Technology. He can render the Committee especially valuable service in connection with various governmental relations. My interest in the Committee work is as keen as ever, but my value to the Committee is necessarily small as compared with that of Dr. Stratton. . . . There is not the slightest hesitation on my part to withdraw from the Committee, in order to provide a place for Dr. Stratton, even though I have greatly enjoyed my association with the Committee for eight years, have greatly prized the honor of being on the Committee, and even though I shall have a keen sense of a personal loss to me as I drop out."

Professor Hayford's resignation from the Committee was formally accepted by President Warren G. Harding under date of May 26, 1923. President Harding wrote:

"I desire especially to commend your fine spirit of public interest in sacrificing your membership in the National Advisory Committee for Aeronautics in order to promote the Committee's welfare by making possible the continued membership of Dr. S. W. Stratton, who is Secretary of the Committee.

"In accepting your resignation, I wish to express to you the thanks of the Government for your patriotic service without compensation as a member of the National Advisory Committee for Aeronautics since its organization in 1915."

Dr. Charles D. Walcott, Chairman of the Committee, wrote to Professor Hayford on May 24, 1923, expressing regret for

". . . the situation that is the cause of the severance of our official relations, which have been most cordial and delightful since the organization of the Committee in 1915. I know that this action has caused you regret that can be overcome only by the satisfaction that comes from doing the big thing—from making a personal sacrifice for the general good, a sacrifice not unlike that which has always characterized your service on the Committee during the past eight years."

As might be expected, Professor Hayford's connection with the National Advisory Committee for Aeronautics and with the Bureau of Standards on aviation problems soon brought him to the attention of the public at large and he was invited often to address various scientific or civic bodies on subjects connected with aviation, and his services were often employed on committee work in engineering societies. A partial list of these is given here:

Chairman of the Aviation Committee of the Western Society of Engineers, 1920.
Representative of Western Society of Engineers on Air Board of Chicago, appointed April 19, 1922.
Member of the Aviation Committee of Western Society of Engineers, appointed June 17, 1922.
Address—"What American Science is Doing for Aviation."
  Chicago Academy of Science, January 25, 1918.
  Sigma Xi at Northwestern University, February 4, 1918.
  Western Society of Engineers, February 5, 1918.
Address—"The Future of the Airplane."
  Cosmos Club of Washington, January 5, 1920.

Address—What Should Be Done to Develop the Civil Use of Airplanes?"
Western Society of Engineers, 1920.

VISITING COMMITTEE OF THE BUREAU OF STANDARDS

On February 26, 1912, Professor Hayford was appointed by the Secretary of Commerce and Labor as a member of the Visiting Committee of the National Bureau of Standards at Washington, D. C., the appointment to be for four years. He was absent from Evanston on this duty about one week each year.

In accordance with Section 10 of the Act of Congress, approved March 5, 1901, to establish the Bureau of Standards, "The Visiting Committee shall consist of men prominent in the various interests involved and shall visit the Bureau at least once a year, and report to the Secretary of the Treasury upon the efficiency of its scientific work and the condition of its equipment."

The other members of this committee, besides Professor Hayford, were Professor Elihu Thomson, Professor Henry M. Howe, President R. S. Woodward, and Professor A. G. Webster.

Under date of June 9, 1917, Professor Hayford was reappointed a member of the Visiting Committee of the Bureau of Standards. The appointment was signed by Hon. William C. Redfield, Secretary of the Department of Commerce, and was for the period beginning June 9, 1917, and ending June 30, 1921.

WAR WORK AND THE BUREAU OF STANDARDS

When the United States entered the Great War it was but natural that Director John F. Hayford of the College of Engineering at Northwestern University should be called into service at Washington because of his former connections there in various capacities and because of the fact that he had been a member of the National Advisory Committee for Aeronautics since 1915. He responded to the call in June, 1917, immediately after Commencement at the University. He maintained close touch with

the College of Engineering by coming back to Evanston more than twenty times during the extent of the war, for periods varying from one to six weeks. The University was thus able to help in the war work by lending his services and without losing his help at Evanston.

His Washington work was the result of a close cooperation between the National Advisory Committee for Aeronautics and the Bureau of Standards, as both supported the investigations which were carried on.

In June 1917, when he entered the work, he was given a roving commission to help, anywhere he saw fit, in the investigations connected with aeronautics which were then in progress at the Bureau of Standards. His appointment to this work was in the nature of a temporary one as Associate Engineer Physicist, Bureau of Standards, and was to continue for a period not to exceed three months. Under dates of October 11, 1917, and January 8, 1918, respectively, this temporary appointment was extended by a similar three months' temporary appointment.

He soon reached the conclusion that the particular field in which he could be of most value was in the development of the necessary instruments and methods for making tests of full-sized airplanes in free flight in the normal manner. Nothing was then being done toward the development of such tests as were contemplated.

At that time airplanes, airplane propellers, and airplane engines were designed mainly on the basis of tests made on the ground. Full-sized engines were used on these tests, but some of the conditions which the engines meet in the air could not easily be duplicated on the ground. In so far as the airplane propellers and wings are concerned, the designs were based mainly upon information gained from observations upon small models usually not more than thirty inches in the longest dimension. These models were tested in an artificial wind in a wind tunnel, and from them conclusions drawn as to the probable performance of full-sized propellers and wings in the air.

It was recognized to be extremely important to secure the exact information as to the performance of the full-sized machine

in the air. Such tests as had been made in the air were either very incomplete or the accuracy much lower than was recognized to be necessary. The reason that the free-flight tests of the required kinds had not been secured was simply that the difficulties in the way were so great that but few persons had attacked the problem, and these few had necessarily made but small progress. It was recognized, however, in June 1917 that the importance of such tests was so great that they should be made at any reasonable cost, and both the Advisory Committee and the Bureau of Standards were prompt in offering support to the project. The proposed tests would furnish decisive evidence as to the strong and weak points of a given airplane, including its engine and propeller, and so would furnish a firm basis for progress in making improvements.

To make the desired tests in a satisfactory manner it was necessary to devise six new instruments which would record autographically and continuously in the air the following six quantities:

(1) The number of revolutions per minute of the engine,
(2) The torque of the engine (this being the quantity which, multiplied by the revolutions per minute, furnishes the horse-power),
(3) The thrust of the propeller,
(4) The inclination of the airplane wings to the true horizon as it flies,
(5) The speed of the machine through the air, and
(6) The inclination of the actual path of the machine.

Such records would show continuously the power developed by the engine and its efficiency, the efficiency of the propeller, the efficiency of the wings, and they would show under what conditions the performance of the airplane and of its separate parts was best and under which it was poorest during the flight.

It was necessary that each record should be autographic, that is, that the instrument should draw a continuous line on a moving sheet of paper to indicate the fact at every instant. Most of the instruments to this date were instruments which the observer or pilot necessarily read by eye, by looking at a

pointer against a dial. If the attempt was made to secure the free-flight tests with such dial-reading instruments the results would not be satisfactory, for though an observer made a reading every ten seconds, the airplane would probably have traveled more than a mile before one reading of each of the six instruments had been made. During that time the conditions would probably have changed so that the readings of the different instruments would have been made under different conditions. With the autographic instruments all of the six records might be read at leisure on the ground for the same instant of flight, so that corresponding values of the different quantities could be obtained.

As the work progressed, Dr. Lyman J. Briggs of the Bureau of Standards and Professor F. W. McNair of the Michigan College of Mines were associated with Professor Hayford in the creative part of the work. At various times many others were helping in the work. An English officer in Washington who had made tests in England which were then the nearest approach to the required free-flight tests furnished valuable advice.

A new, original design was found to be necessary for each of the six instruments. At the outset four of the six appeared to be nearly impossible. One of the instruments has proved to be a new invention which was patented in favor of the United States. This was a gyroscope apparatus, and was patented under date of September 19, 1922, with John Fillmore Hayford and Lyman James Briggs as assignors to the Government of the United States. All of the six were finally completed and were built in five different shops, three in Washington, one in Brooklyn, and one in Champaign, Illinois.

Early in April, 1918, it was recognized that one of the six instruments, which had then been demonstrated, had several war applications other than the application to free-flight tests. As a result Mr. Hayford and his group were required to turn aside and design modified forms of the instrument for two or more war applications. Though these modified instruments were not finished before the Armistice was signed, the military authorities decided to have them completed, which was done after the

Armistice. Thus eight new instruments were the outcome of the work by Mr. Hayford and those associated with him. The material that Director Hayford left regarding these is marked "Confidential," and though war conditions no longer prevail, the writer, for obvious reasons, does not divulge the nature of these instruments. It may be that this precaution is no longer necessary.

On July 19, 1919, Professor Hayford, together with Professor McNair and Dr. Briggs, his associates in the development of these special instruments as modified for the War and Navy Departments, left Hampton Roads, Virginia, with the Pacific Fleet enroute to San Diego, California, by way of the Panama Canal for the purpose of carrying on tests of the instruments for the Navy under actual sea conditions. These tests were made on board the U. S. S. Mississippi. Commander W. R. Furlong, U. S. N., of the Bureau of Ordnance, accompanied the party, made all arrangements with the officers of the Mississippi, and took an active part in all tests. Captain Moffett and the officers of the Mississippi furnished the party with every facility desired, and Admiral Hugh Rodman in command of the Pacific Fleet gave the Mississippi permission to drop out of the fleet formation every day on much of that part of the trip which lay in the Pacific. The tests were carried on during practically the entire trip which ended at San Diego, California, on August 5, 1919. From ten thousand tests made on this trip it was proved that the instrument was a practicable, convenient, reliable instrument for indicating the position of a ship with reference to the true horizon, regardless of the visibility conditions of the sea-horizon.

Letters of appreciation for the services of Director Hayford during the war were received by President Lynn Harold Hough of Northwestern University from the Navy Department and from the Bureau of Standards. Dr. Stratton, of the Bureau of Standards, wrote:

"Professor Hayford and his associates have not only solved to the satisfaction of the Navy Department the problems pre-

sented, but have secured data and developed devices which we now see will have many important applications in aviation, in navigation, and in the industries."

Similarly, in expressing its appreciation of the services of Professor Hayford, the Bureau of Ordnance of the Navy Department stated that the work done was a great contribution to the efficiency of the gunnery of the United States Navy and that the Bureau "particularly appreciates the generosity and patriotic spirit of the officers of your institution (Northwestern University) in allowing Dr. Hayford to give considerable time and effort to the government. You may be recompensed in knowing that the time of Dr. Hayford thus given has resulted in efficiently solving a very important scientific problem for the United States."

ROCKAWAY POINT

In 1921 Mr. Hayford was called to New York City to serve as Expert Witness in the case of the Rockaway Pacific Corporation against the State of New York. It was an interesting case of a point of land which in eighty years had extended itself more than three miles into an area formerly covered by the waters of the ocean from depths of ten to thirty or more feet, and is probably the most rapidly growing point anywhere on the shores of the United States. Mr. Hayford's function was to help establish the facts as to how Rockaway Point near New York City had grown. He spent considerable time gathering information at the Coast and Geodetic Survey and other places, and finally testified before the court at Utica, New York, January 22-26, 1921.

THE SLIDES OF THE PANAMA CANAL

In 1915, and for some time before, the use of the Panama Canal was badly affected, due to the occurrence of sliding or caving in of the banks at several points along the Canal. At the request of President Woodrow Wilson on November 18, 1915, the National Academy of Sciences appointed a committee of thirteen persons "to consider and report upon the possibility of

controlling the slides which are seriously interfering with the use of the Panama Canal."

In 1912 Professor Hayford, while in Central America in connection with the Costa Rica-Panama Boundary Survey, had the pleasure of spending five days at the great Culebra Cut with Col. Gaillard, accompanying him through the cut on his regular inspection trips each day, climbing three of the slides which were then active, and hearing him discuss the nature and cause of the slides. Also in that year he became well acquainted with Mr. Donald F. MacDonald, the geologist employed especially to study the slides.

During the four years, 1912-1915, as he states, he "read with avidity" everything he saw in regard to the Panama slides and his voluminous scrap book bears ample evidence that this was no idle statement. Therefore, when the invitation came to be one of the Committee to study this great question he responded most heartily, his acceptance being dated November 26, 1915.

The Committee as originally appointed consisted of thirteen persons, as follows:

C. D. Walcott, Scientist, Secretary of the Smithsonian Institution.

G. F. Becker, Geologist, in Charge of Division of Chemical and Physical Research, U. S. Geological Survey.

R. S. Woodward, Scientist, President, Carnegie Institution of Washington.

Arthur L. Day, Physicist, Geophysical Laboratory.

C. R. Van Hise, Geologist, President, University of Wisconsin—Chairman of the Committee.

H. L. Abbott, Army Officer and Engineer. Long record as investigator in connection with the Mississippi River and Panama Canal.

J. C. Branner, Geologist, recently President of Stanford University.

Whitman Cross, Geologist, U. S. Geological Survey.

R. C. Carpenter, Professor, Mechanical Engineering, Cornell University.

A. P. Davis, Chief Engineer, U. S. Reclamation Service. Twice before called into service in connection with the Canal.

J. R. Freeman, Consulting Engineer, Providence, R. I. Once before called into consultation on Panama Canal Problems.

J. F. Hayford, Civil Engineer, Director, College of Engineering, Northwestern University.

H. F. Reid, Professor of Geology, Johns Hopkins University. Eminent student of glaciers and earthquakes.

For various reasons the first three were unable to visit the Canal and participate in the deliberations of the Committee leading to the preliminary report; the fourth declined service on the Committee.

The remainder of the Committee sailed from New Orleans, December 11, and arrived at Panama, December 19. All spent two weeks in the Canal Zone, and three of them several days longer, working upon the problems submitted to them. The Committee saw the Canal from end to end, but directed its main attention to the slides and hills of the Culebra District, where the engineers encountered the most serious difficulties. The work of the Committee in the field was facilitated in every way by Major General George W. Goethals, and many officers and engineers connected with the Canal work, and by the work and services of Mr. MacDonald, Canal Geologist from 1911 to 1913.

It was desirable that President Wilson and Major General Goethals should have the general conclusions of the Committee as early as possible; and, accordingly, a preliminary report was prepared, which was placed in the hands of the President on February 3, 1916. It was published in the *Proceedings of the National Academy of Sciences,* 1916, Vol. II, pp. 193-207, and in the *Annual Report of the Isthmian Canal Commission* for the fiscal year 1915-16, pp. 587-98.

Meetings of the Committee were held in Washington, D. C., April 22, 1916, and in Boston, November 11, 1916, to discuss and formulate the final report which was signed by Professor Hayford March 24, 1917. On consideration of all available data the Committee modified some of the conclusions and tentative recommendations presented in the preliminary report. Although

presented too late to be of much use in the "control" of the active Culebra slides, it was hoped that this final report of the Committee would lead to a more thorough understanding of the slide phenomena and more complete future protection for this great thoroughfare of commerce.

The final report is contained in Volume 18 of the Memoirs of the National Academy of Sciences, published in 1924.

At the time of activity of the slides they were a problem of great interest to the general public as well as to the scientific men of the world. As might be supposed, Professor Hayford was besieged from all sides for information regarding them, and organizations of various kinds invited him to address them on the subject. It was not, however, until after the report was placed in the hands of the President in February, 1916, that he consented to talk or write for public use regarding the slides. He had prepared from official photographs a large number of slides and these were used to illustrate his address on "The Great Land Slides at Panama."

A BUSY MAN

During the war extra burdens were taken over by Hayford. It must have been, indeed, a busy time for him. On November 2, 1917, he writes:

"It is important to the success of the United States in war, if it is to be a long war, to keep the universities going, and still more important to the United States in the reconstruction period after the war that the supply of trained thinkers should not be allowed to diminish any more than is absolutely necessary. My belief in these propositions is shown by the determination with which I am sticking to my work as Director at the same time I am trying to do my part in Washington."

And again on September 8, 1920, he says:

"I am now thoroughly immersed in my college duties plus original researches in connection with gyroscopes, aeronautics, and evaporation and stream flow. This is a heterogeneous list which is a result of following where duty and opportunity both lead. I spent much of my time for three years in Washington

on war duty while still carrying my responsibilities at Northwestern University. Incidentally, during these three years I have travelled about 40,000 miles, including twenty-one round trips between Evanston and Washington."

ROTARY GRAVIMETER

Among the many scientific problems in which Professor Hayford was interested was that of determining the intensity of gravity at sea. It was one of the outstanding problems, as he saw it, whose solution was needed in order to complete satisfactorily the studies of gravity and isostasy. He was continually issuing a challenge for some one to invent a device by means of which the intensity of gravity at sea could be determined with an accuracy commensurate with those made on land, and he, himself, pondered long on the possibility of such a device.

In 1921 he was confined to a hospital in Evanston for a minor operation. On the occasion of one of the visits which the writer paid him during his convalescence, he found Professor Hayford in a very jubilant mood, and was shown the preliminary studies of a device which seemed to have promise of solving this important gravity problem. The device was an adaptation of the principle of the rotating governor and the displacement of the arms was to be a measure of the force of gravity. For this reason Professor Hayford termed it "the rotary gravimeter," and for a year or more the study of this took up much of his spare time.

In December of that same year he wrote to Lyman J. Briggs: "I am still finding nothing wrong with the scheme and am accumulating evidence in favor of the probable success of the instrument."

He deduced a complete theory for the instrument and made computations as to its accuracy and possibilities.

In a letter to William Bowie written in February, 1922, he wrote: "After studying more than one hundred and thirty hours on the problem I found that my proposed instrument will not serve the purpose. I have killed my own bright idea by proving that it will not work." Under date of February 13, 1922, he

abandoned its further study and marked the seventy-five pages of the study with these words: "The Rotary Gravimeter is not feasible for determining gravity at sea," and then proceeded to give a complete summary of the reasons therefor.

This must have been a severe blow to him, for it was a problem whose solution was so ardently desired. It is the only case known to the writer where Professor Hayford failed to arrive at his goal within a brief period. The studies which are contained in the seventy-five pages constitute a masterpiece of intrinsic reasoning and mathematical analysis, and the instance is here cited because it shows very clearly his method of attack and his mental processes in carrying out a specific line of investigation. Although failure was the reward for his many hours of work, this study shows the truly wonderful research qualities of the man and the splendid analytical powers he possessed.

COLORADO SCHOOL OF MINES

On May 31, 1915, the Board of Trustees of the Colorado School of Mines, Golden, Colorado, extended an invitation to Professor Hayford to consider himself as a candidate for the Presidency of the Colorado School of Mines. This invitation was extended by Mr. H. C. Parmlee of the Board upon recommendation of Dr. Richard Maclauren, President of Massachusetts Institute of Technology. On June 4, Director Hayford wrote that he did not care to be considered as a candidate for the position, stating that "Northwestern University is treating me so well and there is so much need for my continued presence here in this young College of Engineering that I do not care to consider leaving." Upon being informed of this decision of Professor Hayford, Dr. A. W. Harris, then President of Northwestern University, expressed himself by writing: "Your letter with its enclosures pleased me immensely, first because of the compliment to you and the reflected compliment to Northwestern, and second, because of the great compliment to Northwestern contained in your decision. To lose the Hayfords from Northwestern would be a loss so serious that I do not find it easy to characterize it."

SOCIETY FOR THE PROMOTION OF ENGINEERING EDUCATION

Long before he became Director of the College of Engineering at Northwestern University, Mr. Hayford showed great interest in the Society for the Promotion of Engineering Education, and joined with the group of teachers and workers in the engineering field who were active in studying the possibilities of improving engineering education. He became a member of the Society in 1905, and throughout the following twenty years was a most ardent worker in this cause, and very markedly so after his appointment to Northwestern.

While still in the government service he gave two talks before the Society, one "Opportunities for Engineering Graduates in Government Service," and the other, "Why not Teach About Men, the Most Important and Difficult Tools an Engineer Uses?," published in Volumes XIII and XIV of the Bulletin of the Society.

He seldom was absent from any of the annual meetings of the Society and usually took part in some way, either by presenting a paper or by entering into the discussion of papers presented. At the Ames, Iowa, meeting in 1915, he presented the paper, "Reflections of a Director," and at the Baltimore meeting, 1919, "Reflections of an S. P. E. E. President," and was co-author on "Fifteen Years' Experience with a Five-year Engineering Curriculum at Northwestern University," presented by the writer at the Boulder, Colorado, meeting in 1924.

He served the Society in many capacities; was on its Council in 1909, in 1916, and again in 1921, was Vice-President in 1917-18, and President in 1918-19. During 1918-19 he was a member of the Program Committee, in 1918-19 and 1919-20 a member of the Publication Committee, and in 1918-19 a member of the Executive Committee. These were added to the heavy duties which he had in connection with his work in Washington during the War, and to his duties as Director of the College of Engineering.

Through his invitation Northwestern University was host to the Society in 1918. Due to the fact that President Ketchum

was not able to be present, Director Hayford served as President during that meeting and was at that time elected President for the ensuing year.

In December, 1917, he was appointed by President Ketchum as a member of a committee of eighteen to secure a ruling from the Provost General of the United States Army in regard to enlisting engineering students in the Enlisted Reserve Corps so that they might finish their course before entering active service. This was one of the prime factors in the organization of the Engineer Enlisted Reserve Corps during the World War.

In December, 1918, Director Hayford presided and gave the address of welcome on behalf of the Society for the Promotion of Engineering Education at the joint meeting of the Society and the British Educational Meeting of the American Council on Education, held at the Massachusetts Institute of Technology, Cambridge, for a discussion of engineering education in relation to the war.

In his address as retiring President, given at Baltimore in 1919, he called attention to the rare opportunity he had had of presiding over three successive meetings, an opportunity not given to any of his predecessors.

At the request of the President of the Society, he represented the Society for the Promotion of Engineering Education at the Congress on Public Information held at the Congress Hotel, Chicago, in February, 1921, and acted as Chairman during one session of the Congress.

He was one of the Deans of Colleges of Engineering, men with long experience in engineering education, men proud of the progress already made and imbued with the desire to make still further progress in improving engineering education, who met by appointment for a conference in Chicago in May, 1922. These deans, in conference assembled, passed the now famous resolution:

"Resolved, that in order to meet the constantly enlarging responsibilities of the engineering profession we favor an advance in engineering education at this time that shall provide for five years of collegiate training for those engineering students whose

aim is to be qualified to take positions among the creative leaders in the profession."

During the time that engineering education was being studied by C. R. Mann, under a grant from the Carnegie Foundation, and later by Mr. Wickenden, Director of the Board of Investigation and Coordination, Mr. Hayford took a keen interest in these studies, and many times called his faculty together informally to discuss the various phases of engineering education brought to light by these studies and investigations.

In regard to Director Hayford's work in the Society for the Promotion of Engineering Education, Dean F. L. Bishop, Secretary of the Society, wrote to the writer under date of January 30, 1929:

"He was always ready with advice, and could be depended upon to do whatever job was handed him. His clear mind and direct way of thinking was of great value to all members of the Society who had the pleasure of knowing or hearing him. His organization of the Baltimore meeting in 1919 was one of the outstanding features of the work of the Society immediately following the war. He was of great assistance during the war in keeping the work and activities of the Society on a going basis."

HAY-FEVER

In general Professor Hayford enjoyed abounding good health, but he had one affliction which bothered him very much at times, so much so indeed that it was a most pitiful sight to see him trying bravely to bear up under it. This was hay-fever. In regard to this he wrote under date of December 19, 1919:

"Ordinarily I have had a form of asthma each year as a climax to the hay-fever. Ordinarily this asthma is so severe for two weeks or more as to make it difficult or impossible to sleep continuously except in a sitting position rather than lying down. I have had hay-fever every summer since 1895, in some summers in a very severe form, have never run away from it, have been in various parts of the United States during the hay-fever season and have studied the disease carefully."

To the writer's knowledge he did bear up bravely under it, and was usually found at his desk or at whatever work he was engaged in, keeping regular hours, and striving by frequent use of menthol or other aromatic unguents to minimize the discomforts attendant upon this irritating affliction.

PERSONALITY

During the early years of the writer's acquaintance with Mr. Hayford, the most impressive characteristic was his apparent happiness. His jovial, hearty laughs would ring out merrily along the halls of the old Coast and Geodetic Survey building. He had a generous fund of humor and this was one of the ways he expressed it. His aptitude for seeing the humorous, and the shrewd turning of a phrase, or the "cracking of a joke" was noticeable to all.

This characteristic remained for some time after he became Director of the College of Engineering, but gradually a change became apparent. It may be the advancing years were causing him to be more sedate, but it is more likely that the load he was carrying as Director, administrator, teacher, research investigator, student advisor, his many addresses—all added their part to make him more sedate and less inclined to laugh.

In later years his brain must have been called upon for an abnormal amount of work and from the records that he has left in his voluminous correspondence one is lost in wonder that he could have so successfully kept the many strands of thought from becoming hopelessly entangled.

In gathering the material for this memoir nothing has impressed the writer more than the enormous amount of work which he accomplished and of which little was known, even to those who were closely in touch with him. The fields in which he labored were so diversified that his friends and associates and colleagues, each knew only a small part of the activities in which he was engaged. In looking over his correspondence one could easily gain the idea that his list of correspondents included nearly every man in *Who's Who* in engineering and the related

sciences and engineering education. It was a tremendous task for one man to accomplish.

He seemed to have an unfailing fund of energy upon which he constantly drew, and an industry which never seemed to need periods of idleness to refresh him. An inextinguishable fire of energy and enthusiasm was his, and even to the last his mind was on some scientific problem. But he had that most happy faculty of being able to lay aside his work when quitting time came and taking his relaxation in the form of a walk along the lake shore, or in a good game of volley ball or a game of "cowboy" at the club, and in play he always put the same abounding energy and enthusiasm as in his scientific work. In nothing he undertook did he do half-hearted work, but drove himself forward along his adopted course with a vim and vigor which astounded all who knew him.

His activities penetrated into practically every field of the physical sciences and he was keen to travel along new paths of exploration. The list of learned and civic societies to which he belonged is a long one and in these he gave to the fullest of his time and energy. The list could have been lengthened several times had he responded favorably to the many invitations offered him, but it was a firm conviction of his that he did not care to join an organization for the sake of the mere honor, and unless he saw that he could prove worthy of the membership by giving to it a worthwhile service, he politely but firmly declined to have his name considered for membership.

CHURCH

The writer does not know the strength of Hayford's religious convictions. He was a member of the Unitarian Church at Washington, and later affiliated with All Souls Unitarian Church when he moved to Evanston. Evidently, as in everything else into which he entered, he took a very active part in the work of the church, for the church in Evanston has honored his memory by dedicating one of the rooms of the church as "Hayford Hall." He was also head of the Layman's League of the Church.

Though devoted to his work, he did not live aloof from his fellow citizens, but always took a keen interest in matters affecting the welfare of the city. This extended to political questions, as well as civic improvements and civic organizations.

PERSONAL APPEARANCE

In personal appearance he was of medium height, but strong and compactly built, and in later life rather heavy in build and slow in motion. His pictures taken at Anchorage Bay, Alaska, in 1894, show him with a full beard. He wore a full beard up to about the summer of 1905, after which he remained smooth-shaven.

EDUCATION

When asked to tell of his education he responded in 1923 with the following:

"To the present time my education has been that represented by the degree of C.E. from Cornell University, and Sc.D. from George Washington University, supplemented by seventeen years of education received from students, three years while acting as instructor at Cornell University (1895-1898), and fourteen years (1909-1923) while acting as Director of the College of Engineering at Northwestern University; and extended by seventeen years in the service of the United States. The majors, so to speak, in that education have been an investigation of isostasy for thirteen years, and an investigation of evaporation and stream-flow for nearly twelve years, and which is still in progress. There is still hope that my education is advancing to a perceptible degree."

DOCTOR OF SCIENCE

On June 5, 1918, Director Hayford was granted the honorary degree of Doctor of Science by George Washington University at Washington, D. C. The statements made when the degree was conferred indicate that it was intended as a recognition of the scientific work that he had done in connection with the United

States Coast and Geodetic Survey and in other parts of the government service.

In an alertness test, conducted at the Rotary Club of Evanston, March 20, 1924, by L. B. Hopkins, Director of Personnel at Northwestern University, Hayford obtained a score of 71. Of the 75 men tested by Professor Hopkins at the Club the average was 46.

VICTORIA MEDAL

It is somewhat difficult to pick out the most significant honor which came to Professor Hayford. Standing among the highest, however, should be listed the award to him of the Victoria Medal of the Royal Geographical Society of Great Britain. Notification of this award was sent to Hayford under date of March 5th, 1924, just one year before his death.

The Secretary of the Society, Arthur R. Hinks, in conveying the notification wrote: "I am happy to inform you that the Council have awarded you the Victoria Medal of this Society for your establishment of the theory of Isostasy. The medal will be presented at the Anniversary General Meeting on May 26. It is, I suppose, too much to hope that you will be in England at that time, but I shall be glad to hear from you whether you would wish to nominate any personal friend to receive the medal for you or whether we shall ask one of the Embassy.

"May I take the opportunity of expressing to you the great pleasure which it gives me personally of being the medium of conveying to you this decision of the Council."

As Mr. Hayford was not able to be present on May 26th to receive this medal, it was awarded in his absence. As stated in the Geographic Journal (V. 64, p. 83) "The Victoria Medal was presented to Mr. Boylston Beal—in the unavoidable absence of Mr. Hayford himself and of His Excellency the American Ambassador." Announcement of the King's approval of the award was made in the April issue of the Geographic Journal (V. 63, No. 4, April 1924, p. 361). In so far as the writer has been able to find out this signal honor has been granted to

but two other Americans in recent years—Commodore Peary * in 1910, and Alexander Hamilton Rice * in 1914.

MOUNT HAYFORD

About two years before the death of Dr. Hayford, Colonel E. Lester Jones, as American Commissioner on the International Boundary, submitted to the United States Geographic Board through the State Department the proposal that the name "Mount Hayford" be given to one of the peaks in southeastern Alaska, with the hope that the Board would waive the rule against naming geographic features for living persons in this instance because of the eminence of Dr. Hayford in the scientific world.

At this time the Board did not concur in the recommendation, but on May 6, 1925, shortly after Dr. Hayford's death, the recommendation of Colonel Jones was formally and unanimously approved, and the name "Mount Hayford" was inscribed on the map of the world for all time.

In a letter to Mrs. Hayford apprising her of this action of the Board, Mr. James W. McGuire, Member of the U. S. Geographic Board, states, "The peak itself rises in grandeur, one and one-quarter miles toward the heavens, its snowy summit only six miles from the seashore; a monument raised by the hand of the Creator."

Mount Hayford is six thousand four hundred and forty feet high, and is located about six miles west of Portland Canal, southeastern Alaska, near latitude 55°44′.

STUDY MEN

Of his non-technical writings few attained the recognition accorded to "Study Men," originally given as an address deliv-

* In 1898 Commander Peary was awarded the Patron's Medal of the Society and in 1910 was awarded a Special Gold Medal. Dr. Rice was awarded the Patron's Medal in 1914. The Gold Medals of the Royal Geographical Society, the Founder's Medal, and the Patron's Medal, are not identical with the Victoria Medal received by Mr. Hayford. (Geographic Journal, V. 64, 1924, pp. 81-83.)

ered on Commencement Day, June 14, 1907, at the Thomas Clarkson Memorial School of Technology, Potsdam, New York, and published in the Clarkson Bulletin, July, 1907, and later incorporated in *Addresses to Engineering Students* by Waddell and Harrington.

Some of the thoughts contained therein are worthy of note here.

"One of the prominent characteristics of the average engineer is that he is so wrapped up in his work as to see only its immediate results and to fail to see its much great indirect effect. He fails to realize fully that he is working through men and for men, that the most important effect of his work is its influence upon the onward and upward progress of man."

He urges the engineering student to study men "because much of your learning is done through other men, because you will do your work through men, and because men are so difficult to understand. Men are the most important objects of interest that will come within your sphere of knowledge. I urge you to study men because I am safe in saying that there are some of you who will fail to be useful in the world simply because you will fail to understand men until it is too late. The effectiveness with which you will use your engineering knowledge depends very, very intimately upon your knowledge of men. You are urged to pay attention to all phases of men around you, to see and appreciate them as literary and artistic men as well as technical men, as men of feeling as well as men of thought, as incarnated motives as well as thinking and working machines."

The thoughts embodied in this address seemed to be one of the guiding forces in his life. He was not a recluse, a solitary man, a man hiding himself in a cloister. He delighted in the exchange of thought whether with the highest scientist of the land or the humblest workman on the surveying parties. At meetings of scientific societies he considered the time well spent if he could get in close touch with some new personality, even though the business of the meeting seemed to have little of profit in it for him. And these "touches" were no passing, idle contacts for him. They generally contained some pertinent fact

which his wonderful memory would bring to light and make use of in the future.

"Study Men" was probably at the foundation of his organization of one of the courses taught by him at Northwestern University. This was the course in "Public Relations of Engineers," which is discussed in another part of these memoirs.

LAST DAYS

As mentioned before, the last public appearance of Professor Hayford was at the Annual Banquet of the Engineering Society of the School of Engineering held at Evanston on the evening of December 19, 1924, at which he gave a most interesting talk of the court reception given by the King to the delegates to the Fifteenth general conference of the International Geodetic Association held at Budapest in 1906. So far as the audience could tell, he was apparently in good health, for he stood up all of the time during his talk and gave no evidence of any illness. The next morning, however, he came to the writer's office at the Engineering Building and said that he had been ordered to the hospital for several days' examination. This he did after clearing up some important matters on his desk.

When he made the above announcement it was told in such a way that the stay at the hospital was probably to be only a few days. The examination at the hospital, however, showed that the case was very serious and soon after he was ordered to his home and to have complete rest for some time. Only the family and a small number of close friends were allowed to see him. His condition gradually became worse, and in the early hours of March 10, 1925, Director Hayford passed to the Great Beyond, his death occurring as he slept at his home, 1124 Judson Avenue, Evanston, Illinois. The affliction causing his death was principally dropsical in character, and he suffered quite a bit during those last weeks of his life. In accordance with his will the body was cremated and his ashes spread on Beautiful Lake Michigan, beside the shore of which he had spent so many years and regarding which he had studied so much.

Before going to the hospital he had been under the doctor's care, but to the outside world he gave little indication of this. Evidently his condition had gradually crept upon him, for he continued his daily walks to and from home. But it had been noticeable that he moved less sprightly and vigorously than previously, and seemed to want a more quiet existence.

On the occasion of his death, the tributes received by the family were many, and came from all parts of the country, and some from foreign countries. The writer believes that some of these are so important in showing an estimate of Professor Hayford's life and character that excerpts are here given. Also there have been added a few, some written to Professor Hayford during his lifetime and some written to the writer only recently. Some of these tributes are scattered throughout the memoir, the remainder being given in a group.

TRIBUTES

"I was very proud of his ability as a scientist, a scholar and administrator, but I loved him for himself, so simple and wholesome in his tastes, so kindly and just in his relations with other men and with his students. He was wholesome in everything he did. He had the fun of the boy, and yet the wisdom of a sage.

"I never knew him in all my relations to ask for favors for his school or his own work in the school. Having known the conditions when he took up his work, he accepted them.

"He was a great teacher, because of his scholarship, his clearness."

(Ex-President A. W. Harris, Northwestern University, March, 1925.)

"On behalf of the body generally of Northwestern University the deep and sincere sympathy which they feel at the loss of Dean Hayford. Both because of his very lasting services to the University and his contributions to the sum of the world's knowledge he had endeared himself to the University at large. Because of a peculiarly lovable personality, he was dear to those students who came into contact with him."

(By order of the Student Council of Northwestern University, March 10, 1925.)

"Other graduates of the Engineering School with whom I have talked feel as I do, that all of us have lost a true friend and that the University has lost one of the greatest men that has ever been on its faculty."

(C. D. Hale, March 11, 1925.)

"There is this however that endures, the memory of the inspiring contact that we have had with Professor Hayford and the inspiration of his high standard for the profession."

(Edgar S. Nethercut, Secretary, Western Society of Engineers, April 23, 1925.)

"I am sure that the loss of such an outstanding figure in the field of geodesy is a most serious one and will be very keenly felt."

(Noel Ogilvie, Director, Geodetic Survey of Canada, March 23, 1925.)

"We express our sympathies with the loss which the American Geophysical Union has suffered in the death of this eminent scientist."

(President and Secretary, Norwegian Geodetic Commission, April 27, 1925.)

"He was an active and enthusiastic worker, with great faith and persistence in any idea he took up. His influence was strong in systematizing, and placing on an economically sound basis, the government geodetic work, and in developing important results from it."

(G. R. Putnam, Director, U. S. Light House Service, February 4, 1929.)

"Among us, his friends, there will be cherished in our memories those qualities that won for him a place of unstinted affection. The sparkling eyes, the smiling countenance, that contagious laugh, the kind sympathetic disposition, these all made him so human. Clean-minded, self-controlled in imputing evil to others, he was one of whom it can truly be said that he was master of the greatest of all sciences, how to live among his fellow men."

(Rotary Club of Evanston, March, 1925.)

"I have learned to admire his wonderful attainments and ability, and to look forward to the accomplishment of the splendid

work in science that he has been conducting for so many years. It is impossible to estimate the loss that will result from the sudden termination of his activities."

(C. F. Marvin, Chief of Weather Bureau, U. S. Dept. of Agriculture, March 10, 1925.)

"His going is a great loss to the engineering and scientific work of America, in both of which he did such outstanding work and signal service. He did all things well and the world is better for the useful part he took in it."

(R. L. Faris, Acting Director, U. S. Coast & Geodetic Survey, March 11, 1925.)

"Hayford was a man for whom I had not only a high regard but an affection. I never knew him until he came to the Coast Survey. When I went there in '97 and began to study the institution and its organization . . . one of the first requests I made was for some one who would come as an understudy to Mr. Schott, with the understanding that he would, in a short time, become chief of the division. I was led to think of Hayford because of the admirable work he had done in the Survey and because of some of his papers. At that time he was at Cornell. I got him to come down to Washington and talk the whole matter over with me, and was delighted at the enthusiasm and interest with which he took up his task. The work he did in the Survey not only reflected his admirable ability as a mathematician, but he took up many cognate problems because he was interested in the application of mathematics as well as with mathematical theory.

"During my time in the Survey, Hayford was rapidly improving his mathematical skill and his knowledge of the application of mathematics to geodesy. When Mr. Schott finished the work on the 39th Parallel he gave way very willingly as Head of the Division to Hayford, and from that time on Hayford was the scientific authority of the Survey in all geodetic operations. . . . In my efforts, during the three years I was Superintendent of the Survey, to reorganize in large measure the scientific work I found Hayford a most practical and willing adviser. He had unusual mathematical ability coupled with the quality of clear judgment—a rare combination. I saw nothing of him after he left the Survey but I will always remember him as one of the most able and devoted men with whom I have ever been associated."

(Henry S. Pritchett, President, Carnegie Foundation, June 2, 1930.)

"Five years has not been long enough to reconcile me to his loss."

(A graduate of the School of Engineering writing in 1930.)

"In my administration at Northwestern . . . I think my selection of men was perhaps the most satisfactory part of my service. In no case do I feel more confident than in your own. All this I might be able to say even though the satisfaction were only in a professional and official way, but in your case it is very much more. You have helped to make me satisfied during all the years we worked together and since, as well. I have known many good men and successful men. I put you with a very small group who seem to me to deserve the highest congratulation."

(Ex-President A. W. Harris, Northwestern University, November 21, 1923.)

"Not often is opportunity granted to the engineer to take direct part in unlocking the larger mysteries of nature. Hayford was fortunate enough to find such an opportunity in his work and he was prepared to meet it. Already the truths he developed have had a guiding influence on the work of the geologists in their attempts to decipher the ancient record of the earth's long-past changes, and there is little doubt that the future understanding of the growth and development of our planet will be related very closely to the facts which Hayford brought to light."

(Editorial. *Engineering News-Record,* March 19, 1925.)

The writer had more than a quarter of a century of close association with Professor Hayford, an association which began in the United States Coast and Geodetic Survey in 1899. In these years he found the following characteristics to be uppermost in the life of Director Hayford: intense energy of body and mind, strict honesty of thought and purpose, and a hatred of all show and pretense. He had the great faculty of making and holding friends, and he took especial delight in these friendships. He seldom entered into anything without giving to the fullest of his energies; he got the fullest enjoyment out of life; was a keen lover of all sorts of healthy outdoor sports and indoor games; enjoyed a brisk walk along the lake shore, or an exciting game of "Cowboy" at the Club in company with congenial associates; took an immense amount of pleasure in sitting with the family or

friends reading tales and poems of the old colonies of northern New York and Canada; and withal he had the happy faculty of being able to drop official duties when necessary.

On the scientific side, the ideal set before us by Hayford—his energy, adaptiveness, perseverence, both intellectual and physical, and mathematical precision, are to be held as his most precious characteristics.

He is sadly missed by those who enjoyed his friendship. To the world of science his loss is keenly felt. Those who knew him best esteemed him most, and he has left with them those precious memories of a worth-while life.

On the occasion of the death of Director John F. Hayford, a resolution by the Faculty of the College of Engineering of Northwestern University was adopted March 17, 1925.

WHEREAS

Director John F. Hayford has been the main guiding spirit of this College from its beginning in 1909 to his untimely death one week ago:

Through many years of intimate association the members of this Faculty have come to know him as a man of the following characteristics:

1. He was a believer in the ultimate triumph of truth. He was not a retailer of ready-made opinions and he did not seek to influence others by personal persuasion or by clever statement. On important problems, whether of physical phenomena or of social relations, he sought to find the fundamental facts; to disentangle these facts from matters of emotion and prejudice; and to give these facts clear and forceful statement. He sought to be a leader of men by giving them better views of truth.

2. In mental work he had habits of unusual accuracy, and in industry he was almost indefatigable. In reading he formed the habit of never leaving an article with a hazy notion of its contents; he formulated a statement of its meaning when reduced to simplest terms, and he retained this meaning as a permanent addition to his stock of reliable information. The most of his researches have made successful use of data whose volume and heterogeneity have appeared to other men too forbidding for systematic treatment. His studies of the data of engineering education have been of a thorough and painstaking character.

3. He was thoroughly democratic. In matters of educational policy he had opinions which he believed right because he had

reached them after careful consideration of the facts. But if any question was decided in opposition to his views, he accepted the decision wholeheartedly and carried it into effect with perfect loyalty.

4. He was honest, just, and generous. In college legislation he insisted that every rule should be an accurate statement of policy, and in administration, he undertook to carry out every regulation to the letter of its statement. In the absence of established fact, his opinions were always tempered with extreme generosity; he was not only generous but even prodigal in giving his time and labor to his students and to his friends; in the apportionment of funds to his faculty members he was generous, even self-sacrificing.

5. In manner of life he was modest and unassuming. In general attitude he was genuinely sociable, genial, frank, cheerful, optimistic, loyal, and in important matters aggressive; he had a keen sense of humor; he was slow in the expression of criticism, but prompt in the expression of deserved commendation.

Because of the above characteristics of Director John F. Hayford, the members of this Faculty have great confidence in the general policies which he has advocated for this College.

RESOLVED THAT

This University, this College, and its individual members have suffered a great loss in the death of our friend and director, John F. Hayford, the magnitude of which loss we have hardly begun to appreciate.

The members of this Faculty hope that means will be found not only to continue unabated our efforts in improving engineering education as heretofore directed by John F. Hayford, but so to reinforce our work as to bring to fruition his complete ideal of an engineering college.

It is fitting that these statements of fact in appreciation of our first Director be made a matter of record in our minutes, be transmitted to the family of the deceased, and be transmitted to proper officials of the University for such dissemination and publicity as may be appropriate.

(Signed) WILLIAM H. BURGER, *Secretary.*

In the fall of 1925 funds were collected by the students and faculty of the College of Engineering, and a bronze tablet to commemorate Director Hayford's memory was placed on the

west wall of the lecture room of Swift Hall of Engineering. The tablet reads:

IN MEMORY OF

JOHN FILLMORE HAYFORD

1868-1925

DIRECTOR OF THE COLLEGE OF ENGINEERING OF NORTHWESTERN UNIVERSITY 1909-1925

ESTABLISHER OF THE THEORY OF ISOSTASY

AUTHOR OF THE INTERNATIONALLY ACCEPTED HAYFORD SPHEROID OF REFERENCE

DEDUCER OF THE CONDITIONS WHICH GOVERN THE LEVELS OF THE GREAT LAKES

AN INSPIRING TEACHER—SKILLFUL IN THE USE OF THE SOCRATIC METHOD; DISCIPLE OF ACCURACY AND DEFINITENESS OF KNOWLEDGE; IMPARTIAL INTERPRETER OF FACTS; FIRM BELIEVER IN THE COLLEGE STUDENT; A GENIAL AND MODEST MAN; A GOOD CITIZEN

THIS TABLET IS ERECTED BY HIS STUDENTS

CHILDREN

Besides his wife, he was survived by his four children:

Walter Stone Hayford, born at Ithaca, New York, January 14, 1896. Graduate C.E. degree, College of Engineering, Northwestern University, 1921. Now (1930) in the Research Department of the Bell Telephone Laboratories, New York.

Maxwell Fillmore Hayford, born at Washington, D. C., May 27, 1898. Graduate C.E. degree, College of Engineering, Northwestern University, 1923. Now (1930) Ticket Manager, Athletic Department of Northwestern University, Evanston, Illinois.

John Bryant Hayford, born at Washington, D. C., October 20, 1900. Graduate E.E. degree, College of Engineering, Northwestern University, 1924. Now (1930) Business Manager, Museum of Science and Industry, Chicago.

Phyllis Hayford, born at Washington, D. C., May 18, 1904. Graduate B.S. in Engineering degree, College of Engineering, Northwestern University, 1926. Now (1930) Computer at Lick Observatory, temporarily released and completing work for Ph.D. degree at University of California, at Berkeley, where she has a Teaching Fellowship in the Department of Astronomy.

Mrs. John F. Hayford went to live with her daughter, Phyllis, at Berkeley, California, and died in that city August, 1932.

SCIENTIFIC AND OTHER ORGANIZATIONS

Professor Hayford was a firm believer in the development of a man through contact with men and men's activities. To this end he became a member of numerous organizations and in each of these he gave to the fullest of his powers by presenting papers or in service. His correspondence contains numerous refusals to accept membership in organizations in which he did not see his way to contribute to the welfare of the organization. This stand on his part is clearly indicated in the following quotation taken from a letter (1920) in which he declined accepting membership in a society to which he had received an invitation. "I do not care to belong to any organization in which I cannot do my part. I am already in, and committed to help in so many organizations, that I must restrict my energies to them."

There is given here a list of scientific, educational, and civic organizations with which he was identified. No doubt there are others which have not been listed. The list shows that he was, in truth, "committed to help" in many organizations, and how well he helped is shown by the statement of his activities in so far as could be learned. It is not known whether he continued holding membership in them all up to the time of his death; no correspondence was found among his papers indicating any withdrawals of membership. Perhaps some of them were allowed to lapse.

1. *New York Mathematical Society.*
   Elected to membership, 1891.

2. *Mask and Wig Club of Washington.*
   Member, 1895.

3. *American Society of Civil Engineers.*
   Elected Associate Member, May 6, 1896; member, April 2, 1907.
   Appointed with A. N. Talbot to represent the Society in the Council of the American Association for the Advancement of Science, August 17, 1920.

4. *American Association for the Advancement of Science.*
   Elected member, 1897; fellow, 1898.
   Represented U. S. Coast and Geodetic Survey at Boston meeting, August, 1898.
   Represented U. S. Coast and Geodetic Survey at New York meeting, July, 1900.
   Represented U. S. Coast and Geodetic Survey at Boston meeting, December, 1909.
   On Council, 1910.
   Vice-President, 1910.
   Served as Secretary Section A, Secretary of Council and General Secretary.
   Papers presented:
   "It is not necessary to place geodetic arcs in various latitudes." December 29, 1908.
   "The ellipticity of the earth is not a proof of its former liquid state." December 30, 1908.
   "Relation of isostasy to geodesy, geology, and geophysics." Vice-President's address, Minneapolis, December 29, 1910.

5. *Cosmos Club of Washington.*
   Elected member, December 10, 1898. Non-resident member after 1910.
   Served at times on various committees.
   Made the Cosmos Club his home during his frequent trips to Washington.
   Address—"Future of the Airplane." January 5, 1920.

6. *Philosophical Society of Washington.*
   Elected member, February 11, 1899.

One of the signers of the Articles of Incorporation, May 15, 1901.
Elected Secretary, December 21, 1901.
President, 1907.
Papers presented:
"A new treatment of refraction in height computation." April 29, 1899.
"Recent progress in geodesy." February 3, 1900.
"Determination of the difference of elevation of two given points on the earth's surface." March 31, 1900.
"Corner's apparatus for determining zenith distance of stars that culminate near the zenith." January 5, 1901.
"The new Coast and Geodetic Survey precise level." January 5, 1901.
"Recent progress in geodesy." February 16, 1901.
"What is the center of an area, or the center of a population?" November 23, 1901.
Discussion of "Longitude by wireless telegraphy." January 4, 1902.
"Gravity at North Tamarack Mine, Michigan." October 11, 1902.
"Novel principles applied in rapid primary triangulation." May 6, 1903.
"Telegraphic determinations of longitude of Honolulu." October 10, 1903.
"A test of the transit micrometer." May 21, 1904.
"Computation of the deflections of the vertical from topography." December 10, 1904.
"Geodetic evidence of isostasy." 1906.
"The earth as a failing structure." December 7, 1907.
"The part taken by the Philosophical Society in the development of geodesy."
"The earth from a geophysical standpoint." 1910.
"Relation of isostasy to geophysics." 1911.

7. *American Astronomical and Astrophysical Society.*
Elected member, 1902.

8. *Society for the Promotion of Engineering Education.*
Elected member, 1905.
On Council, 1909, 1916, 1921.
Vice-President, 1917-1918.

President, 1918-1919.
Member Program Committee, 1918-1919.
Member Publication Committee, 1918-1919; 1919-1920.
Member Executive Committee, 1918-1919.
Represented S. P. E. E. at Congress of Public Information, Chicago, February 25, 1921. Chairman, one session.
Papers presented:
"Opportunities for engineering graduates in government service." June 28, 1905.
"Why not teach about men?" 1906.
"Reflections of a Director." Ames, Iowa, June 22, 1915.
"Welcome to British delegates at Cambridge, Mass." December, 1918.
"Reflections of an S. P. E. E. President." Baltimore, 1919.

9. *Washington Academy of Science.*
Elected member.
Vice-President, 1906.
Paper presented—"Geodetic evidence of isostasy." April 14, 1906.

10. *Western Society of Engineers.*
Elected member, December, 1909.
Committee on increase of membership, 1911.
Elected 3rd Vice-President, January 8, 1913.
Chairman, Aviation Committee, 1920.
Member, Student Branch Committee, 1920.
Chairman, Joint meeting with Chicago Section A. I. E. E., November 21, 1921.
Representative of W. S. E. on Airboard of Chicago, April 19, 1922.
Member, Aviation Committee, June 17, 1922.
Member, Committee on St. Lawrence Waterway, November 24, 1923.
Awarded Chanute Medal of Society (posthumous, 1925).
Papers presented:
"Measuring the earth." November, 1913.
"The Great Slides at Panama." March 20, 1916.
"What American science is doing for aviation." February 5, 1918.
"The establishment of isostasy." 1924.

11. *Engineering Society, Northwestern University.*
Member, 1909.
Faculty adviser on several occasions, 1909-1925.
Papers presented:
"Is it advisable to make large expenditures for the improvement of our waterways?" March 10, 1910.
"Measuring the earth." April 22, 1913.
"The urgent need for more engineers." October, 1917.
"Keeping engineering education going at Northwestern University." October 31, 1918.

12. *Washington Society of Engineers.*
Elected member.
Served as Treasurer.
Member, Committee on meetings, 1906.
Papers presented:
"Present methods of precise leveling." January 23, 1906.
"Stream flow." October 15, 1924.

13. *Society of Sigma Xi of Cornell University.*
Elected member.

Society of Sigma Xi of Northwestern University.
Elected member, 1909.
President, 1914-15.
Paper presented:
"What American science is doing for aviation." February 4, 1918.

14. *Chaos Club of Chicago.*
Elected member, November, 1910.
Papers presented:
"Measuring the earth." March 18, 1911.
"The Great Slides at Panama" March 25, 1916.

15. *National Academy of Sciences.*
Elected member, April, 1911.
Appointed member, Panama Slides Committee, 1915.
Papers presented:
"The importance of gravity observations at sea on the Pacific." April 17, 1916.
"Effects of winds and of barometric pressures on the Great Lakes." 1922.

16. *University Union.* (Northwestern University.)
    Elected member, 1909.
    President, 1914-1915.

17. *University Club of Evanston.*
    Elected member, 1909.
    Member, Entertainment Committee, June 16, 1910.
    On Board of Direction, 1923-1925.
    Paper presented:
    "What should be done to develop the civil use of airplanes?" November 19, 1919.
    "The Great Slides at Panama." March 25, 1916.

18. *Illinois State Academy of Science.*
    Elected member, 1909.
    On Council, 1910.
    Paper presented:
    "Relation of pure and applied science to progress of knowledge." February 18, 1910.

19. *Math Club of Northwestern University.*
    Elected member.
    Paper presented:
    "Measuring the earth." January 11, 1922.

20. *American Philosophical Society.*
    Elected member, April 24, 1915.
    Paper presented:
    "The earth from a geophysical standpoint." 1915.

21. *American Physical Society.*
    Elected member.

22. *Astronomical Society of America.*
    Elected member.

23. *Chicago Astronomical Society.*
    Elected to life membership without fees, July 7, 1921.

24. *Rotary Club of Evanston.*
    Elected member.
    Paper presented:
    "Best uses of the waters of the Great Lakes." April 17, 1924.

25. *American Geophysical Union.*
Elected member.
Vice Chairman, Section Geodesy, for two years. 1920.
Member, Executive Committee, Section Geodesy, 1921.
Chairman, section of Geodesy, 1924.
On Executive Committee, 1924-25.
Paper presented:
"Isostasy." April 19, 1921.

26. *Chicago Association of Commerce.*
Appointed member, Rivers & Harbors Committee, January 29, 1924.

27. *Illinois Society of Engineers.*
Appointed judge to pass upon written paper competition, November 23, 1921.

28. *Chicago Academy of Science.*
Paper presented:
"What American science is doing for aviation." January 25, 1918.

29. *National Research Council.*
Member of Geography Committee, 1917.

30. *U. S. Board of Surveys and Maps.*
Member, Committee on Control.

31. *Evanston Academy of Cum Laude Scholarship Society.*
Elected member, June 13, 1916.

MISCELLANEOUS ADDRESSES AND PAPERS:

Detroit High School, commencement oration.
"College Influence." 1885.
Clarkson School of Technology. Founders' Day Address.
"Study Men." 1907.
Dedication Address. Swift Hall of Engineering, Northwestern University.
"The New College of Engineering, An Opportunity." 1909.
Engineering Society, University of Iowa.
"Precise leveling." April 25, 1910.
Engineering Society, State University of Iowa.
"Precise leveling." April 28, 1910.
Engineering Society, University of Wisconsin.
"Measuring the earth." March 10, 1911.

Miscellaneous Addresses and Papers—*Continued*

Men's Club, La Grange, Illinois.
"Panama-Costa Rica Boundary." 1912.
Sigma Xi of Chicago University.
"Measuring the earth." May 22, 1913.
Geographic Society of Chicago.
"Measuring the earth." May 23, 1913.
Michigan School of Mines, class day address.
"The great landslides at Panama." April 14, 1916.
Structural Engineers, Chicago.
"The Great Slides at Panama." October 26, 1916.
Central Association of Science Teachers.
"The Great Slides at Panama." December 1, 1916, Chicago.
Radio Broadcast, WMAQ Chicago.
"Engineering, an occupation which arouses enthusiasm." November 14, 1923.

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1885

College Influence. Commencement Oration. 29th Graduation Exercises, Class of 1885. Detroit High School, Michigan.

1890

Mean Range and Improvement on the Tidal Machine. Coast and Geodetic Survey Report for 1890, pp. 100-138.

1890

On the Least Square Adjustment of Weighings. Coast and Geodetic Survey Report, 1890, Appendix 14.

1890

On the Use of Observations of Currents for Prediction Purposes. Coast and Geodetic Survey Report, 1890, Appendix 14, pp. 691-703.

1890

Comparison of the Predicted with the Observed Times and Heights of High and Low Waters at Sandy Hook, N. J., during the Year 1889. Coast and Geodetic Survey Report, 1890, Appendix 15, pp. 705-14.

1892

On the Least Square Adjustment of Weighings. Coast and Geodetic Survey Report, 1892, Part 2, Appendix 10, pp. 515-27.

1894

An account of Certain Field Methods Used on the Survey of the Mexican Boundary, 1892-1893. Transactions, Association of Civil Engineers of Cornell University, Vol. II, pp. 58-83.

For this paper Professor Hayford received the award of Fuertes Medal, College of Engineering, Cornell University.

1895

The Rueprecht Balance Belonging to the United States Office of Standard Weights and Measures. Coast and Geodetic Survey Report, 1895, Part 2, Appendix 9, pp. 383-92.

1896

The Problem of the Tides, and the Limitations of the Present Solution of that Problem. Transactions, Association of Civil Engineers of Cornell University, 1896, Vol. IV, pp. 31-50.

1898

Limitations of the Present Solution of the Tidal Problem. *Science*, N. S., Vol. VIII, No. 206, Dec. 9, 1898, pp. 810-14.

1898

Geodetic Astronomy. John Wiley & Sons. 351 pp. (a text book) with numerous plates.

1898

Determination of Time, Longitude, Latitude, and Azimuth. Coast and Geodetic Survey Report, 1897-1898, Appendix 7, pp. 261-409. 4th Edition.

1898

Report of the Boundary Commission upon the Survey and Re-Survey of the Boundary between the United States and Mexico West of the Rio Grande, 1891-1896 (pp. 62-128 contain report of the Astronomic Work by John F. Hayford). Government Printing Office.

1898

The Geographic Work of the Coast and Geodetic Survey. *Engineering News*, Vol. XL, No. 22, Dec. 1, 1898, pp. 340-42.

1899

Is There a 428-Day Period in Terrestrial Magnetism? *Terrestrial Magnetism*, University of Cincinnati, March, 1899, pp 7-14.

1899

Precise Leveling in the United States. Coast and Geodetic Survey Report, 1898-1899. Appendix 8, pp. 347-886.

1899

A New Treatment of Refraction in Height Computation. Paper presented before Philosophical Society of Washington, April 29, 1899. Abstract *Science*, May 12, 1899, N. S., Vol. IX, No. 228, p. 686.

1900

The Transcontinental Triangulation Along the Thirty-ninth Parallel. Bulletin, University of Wisconsin, No. 38, Engineering Series, Vol. 2, No. 5, pp. 173-96, plates 1-5.

1900

Recent Progress in Geodesy. Paper presented before Philosophical Society of Washington, February 3, 1900. Published in Bulletin, Vol. XIV, pp. 1-20. Also in *Science*, N. S., Vol. XI, March 9, 1900, pp. 369-92.

1900

The Determination of the Difference of Elevation of Two Given Points on the Earth's Surface. Presented before the Philosophical Society of Washington, March 31, 1900.

1901

A New Connection Between the Gravity Measures of Europe and of the United States. *Science,* N. S., Vol. XIII, No. 330, April 26, 1901, pp. 654-59.

1901

Test of the Massachusetts Institute of Technology Base Apparatus. *Technology Quarterly* Vol. 14, June, 1901. (Co-author with Alfred E. Berton.)

Reprint, *Engineering Record,* Vol. 45, No. 4, January 25, 1902.

1901

Preface to Appendix on the Measurement of Nine Base Lines Along the 98th Meridian. Coast and Geodetic Survey Report, 1901, Appendix 3.

1901

Triangulation Northward Along the 98th Meridian in Kansas and Nebraska. Coast and Geodetic Survey Report, 1901, Appendix 6, pp. 357-423.

1901

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1901

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1901

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1901

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1901

What Is the Center of an Area, or the Center of a Population? Paper presented before the Philosophical Society of Washington, November 23, 1901.

1901

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1902

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1902

Specifications for Triangulation, etc. Coast and Geodetic Survey Report, 1902.

1902

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1902

The Base Line Measurements for the 98th Meridian. *Engineering News,* Vol. XLVIII, No. 10, Sept. 4, 1902, pp. 162-64.

1902

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1902

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1902

Triangulation in Kansas. Coast and Geodetic Survey Report, 1902, Appendix 3.

1903

Triangulation Southward Along the 98th Meridian. Coast and Geodetic Survey Report, 1903, Appendix 4, pp. 811-930.

1903

Opportunities in the Coast and Geodetic Survey. *Technology Review,* Vol. 5, No. 1, January, 1903, pp. 52-57.

1903

Novel Principles Applied and Results Obtained in Recent Rapid Primary Triangulation on the 98th Meridian. Paper presented before Philosophical Society of Washington, May 6, 1903.

1903

The New Coast and Geodetic Survey Level; a Possible Successor of the Wye Level. *Engineering News,* Vol. L, No. 1, July 2, 1903, pp. 2-4.

1903

Report on Geodetic Operations in the United States to the 14th General Conference of the International Geodetic Association. Government Printing Office, 1903, by O. H. Tittmann and J. F. Hayford.

1903

Precise Leveling in the United States, 1900-1903, with a Re-adjustment of the Level Net and Resulting Elevations. Coast and Geodetic Survey Report, 1903, Appendix 3, pp. 189-801.

1903

Recent Telegraphic Determinations of the Longitude of Honolulu, and the Older Determinations from 1555-1903. Engineering News, Vol. L, No. 19, November 5, 1903, pp. 414-15. *Science,* N. S., Vol. XVIII, No. 462, Nov. 6, 1903, pp. 589-93. Paper presented before Philosophical Society of Washington, October 10, 1903.

1904

Determination of the Value of Gravity at the North Tamarack Mine, Calumet, Michigan, from Observations by John F. Hayford and Pres. F. W. McNair, Michigan School of Mines. Unpublished Report, dated February 8, 1904, to the Superintendent of the Coast and Geodetic Survey.

1904

Report on Transit Micrometer Tested During March, April, and May. Coast and Geodetic Survey Report, 1904, 1 page.

1904

A Test of a Transit Micrometer. Coast and Geodetic Survey Report, 1904, Appendix 8, pp. 451-487.

1904

A Test of the Transit Micrometer as a Means of Eliminating Personal Equation. Paper presented before the Philosophical Society of Washington, May 21, 1904.

1904

Precise Leveling from Red Desert, Wyoming, to Owyhee, Idaho, in 1903. Coast and Geodetic Survey Report, 1904, Appendix 6, pp. 403-430.

1904

Precise Leveling from Holland to New Braunfels, Texas, in 1903. Coast and Geodetic Survey Report, 1904, Appendix 7, pp. 433-50.

1904

Recent Practise in the Coast and Geodetic Survey in Primary Triangulation, Base Measurements and Precise Leveling. Eighth International Geographic Congress, September 9, 1904, pp. 531-34.

1904

The Computation of Deflections of the Vertical from the Surrounding Topography. Paper presented before the Philosophical Society of Washington, December 10, 1904.

1904

Surveying. (Co-author with Officers of the Coast and Geodetic Survey.) Part of Proceedings, International Engineering Congress held at St. Louis, Missouri, 1904. Subject No. 35, Paper No. 1. Published by the American Society of Civil Engineers.

1905

A Connection by Precise Leveling Between the Atlantic and Pacific Oceans. *Science,* N. S., Vol. 21, No. 539, pp. 673-74, April 28, 1905; also *Engineering News,* Vol. LIII, No. 11, p. 279 March 16, 1905.

1905

Opportunities for Engineering Graduates in the Government Service. Proceedings, Society for Promotion of Engineering Education. Vol. XIII, 1905, pp. 87-95.

1905

Triangulation Along the 98th Meridian, Lampasas to Seguin, Texas. Coast and Geodetic Survey Report, 1905, Appendix 5.

1905

Precise Leveling from Red Desert, Wyoming, to Seattle, Washington, in 1903-1904. Coast and Geodetic Survey Report, 1905, Appendix 4, pp. 195-241.

1905

The Form of the Geoid as Determined by Measurements in the United States. Report of Eighth International Geographical Conference, 1905, pp. 535-540. Government Printing Office.

1906

The Adjustment of Observations by the Method of Least Squares. 1906. 298 pp. (Joint author with T. W. Wright, and sole author of Chapters VII and IX.) D. Van Nostrand Company.

1906

Present Methods of Precise Leveling. Paper presented before the Washington Society of Engineers, January 23, 1906.

1906

The Geodetic Evidence of Isostasy with a Consideration of the Depth and Completeness of the Isostatic Compensation and of the Bearing of the Evidence Upon Some of the Greater Problems of Geology. Proceedings, Washington Academy of Sciences, Vol. VIII, May 18, 1906, pp. 25-40. Paper presented before the Academy, April 14, 1906.

1906

Geodetic Operations in the United States, 1903-1906. Vol. I of Report of the 15th General Conference of the International Geodetic Association, pp. 192-234. (Joint author with O. H. Tittmann.)

1906

The Budapest Conference of the International Geodetic Association. *Science,* N. S., Vol. XXIV, No. 623, December 7, 1906, pp. 713-19. (Joint author with O. H. Tittmann.) Also *Engineering News,* Vol. LVI, No. 21, November 22, 1906, pp. 540-41.

1906

Why Not Teach About Men, the Most Important and Difficult Tools an Engineer Uses? *Proceedings, Society for the Promotion of Engineering Education,* Vol. XIV, 1906, pp. 198-207.

1907

Report of General Secretary, 57th Meeting of the American Association for the Advancement of Science, Columbia University, December 1906-January 1907. *Science,* N. S., Vol. XXV, No. 628, January 11, 1907, pp. 46-50.

1907

Study Men. Clarkson Bulletin, Vol. IV, No. 3. Commencement Day Address at Clarkson School of Technology. Reprinted in *Electric Journal,* Vol. IV, No. 10, October 1907.

1907

The Earth, a Failing Structure. Bulletin, Philosophical Society of Washington, No. 15, pp. 57-74. Retiring Presidential Address. Read before the Society December 7, 1907.

1907

Study Men, Incorporated in "Addresses to Engineering Students," by Waddell and Harrington.

1907

Geodetic Measurements of Earth Movements. California State Earthquake Investigation Commission Report, Vol. I, Pt. 1, 1908, pp. 114-159. Published by the Carnegie Institution. (Joint Author with A. L. Baldwin.)

Also published as Appendix 3, Coast and Geodetic Survey Report, 1907, pp. 67-104, under different title.

1909

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1909

The Effect of Topography and Isostatic Compensation Upon the Intensity of Gravity. Vol. I, Report 16th General Conference of the International Geodetic Association, pp. 365-89.

1909

Geodetic Operations in the United States, 1906-1909. Report to 16th General Conference of the International Geodetic Association, London and Cambridge, Coast and Geodetic Survey. (Joint author with O. H. Tittmann.) 11 pp.

1909

Precise Leveling in the United States, 1903-1907, with a Readjustment of the Level Net and Resulting Elevations. Coast and Geodetic Survey, 1909, 280 pp. (Joint author with L. Pike.)

1909

Report on the Triangulation of Greater New York. New York Board of Estimates and Apportionments, part of report by J. F. Hayford.

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The New College of Engineering, An Opportunity. *Engineering News*, Vol. 61, No. 20, May 20, 1909, pp. 535-36. Dedication Address, Swift Hall of Engineering, Northwestern University.

1909

Notes on the 1909 Conference of the International Geodetic Association. *Engineering News,* Vol. 62, No. 2, November 11, 1909, pp. 532-33.

1910

The Relation of Pure and Applied Science to the Progress of Knowledge and to Practical Affairs. Presented before the Illinois State Academy of Science, Urbana, Ill., February, 1910.

1910

Is It Advisable to Make Large Expenditures for the Improvement of Our Waterways? Paper presented March 10, 1910, to Engineering Club, Northwestern University.

1910

Precise Leveling. Paper presented before engineering students, University of Iowa, April 25, 1910, and before engineering students, Iowa State University, April 28, 1910. Published, *Iowa Engineer,* June 1910, pp. 144-51.

1910

Theory and Practice of Surveying by Johnson-Smith. John Wiley & Sons, 1910. (Assisted in preparation of chapters on "Triangulation" and "Precise Spirit Leveling.")

1910

Supplementary Investigations in 1909 of the Figure of the Earth and Isostasy. Separate publications, Coast and Geodetic Survey, 1910, 80 pp.

1910

Recent American Precise Leveling. Abstract *Engineering Record,* June 25, 1910, p. 823. Vol. 61, No. 26. Full paper, *Iowa Engineer* for June.

1911

The Relation of Isostasy to Geodesy, Geophysics, and Geology. *Science,* N. S., Vol. XXXIII, 1911, pp. 198-208. Address of Retiring Vice President, Section D, American Association for the Advancement of Science, Minneapolis, 1910.

1912

The Effect of Topography and Isostatic Compensation Upon the Intensity of Gravity. Coast and Geodetic Survey Special Publication No. 10, 1912. (Joint author with Wm. Bowie.)

1912

Geodetic Surveying by Ingram. Review published in *Engineering News,* March 30, 1912.

1912

Isostasy, a Rejoinder to the Article by Harmon Lewis. *Journal of Geology,* Vol. XX, No. 6, September-October, 1912, pp. 562-78.

1912

Panama-Costa Rica Boundary. Paper presented before the Men's Club of LaGrange, Illinois.

1913

The Best Preparation for Engineering. Northwestern University Bulletin, Vol. XIII, No. 21, February 7, 1913, 7 pp.

1913

Report, Commission of Engineers and Panama-Costa Rica Boundary. (Part of Report by John F. Hayford.)

1913

Measuring the Earth. Paper presented before University of Chicago Chapter of Sigma Xi, May 22, 1913. Also before Geographic Society of Chicago, May 23, 1913, the Western Society of Engineers, November 3, 1913, the Mathematics Club, Northwestern University, January 11, 1922, the University of Wisconsin, March 10, 1911, and the Chaos Club of Chicago, March 18, 1911.

1915

Reflections of a Director. Society for Promotion of Engineering Education. Paper presented at the Ames, Iowa, meeting, June 1915.

1915

The Earth From the Geophysical Standpoint. Proceedings, American Philosophical Society, Vol. LIV, No. 219, September 1915, 11 pp. Also Smithsonian Institution, Annual Report, 1916, pp. 239-48 (1917).

1916

Discussion, Col. Townsend's paper on Currents of Lake Michigan. Journal, Western Society of Engineering, Vol. XXI, No. 4, April 1916.

1916

The Great Land Slides at Panama. Paper presented at Class Day Exercises, Michigan School of Mines, Houghton, Michigan, April 14, 1916.

1916

The Importance of Gravity Observations at Sea on the Pacific. Paper presented before National Academy of Sciences, April 17, 1916. Proceedings, National Academy of Sciences, Vol. 2, pp. 394-98, July 1916. Abstract, *Science,* Vol. 20A, No. 715, 1917.

1917

Examples of Accuracy. Compilation of instances of very accurate work done in the Coast and Geodetic Survey. Furnished to Prof. George F. Swain, Massachusetts Institute of Technology, June 11, 1917, for use in book upon the Engineers, being prepared by Prof. Swain. Typed ms. 4 pp.

1917

The Urgent Need for More Engineers. Northwestern University College of Engineering Bulletin, September 24, 1917.

1917

Gravity and Isostasy. *Science,* N. S., Vol. 45, pp. 350-54, 1917.

1917

The Earth From a Geophysical Standpoint. Smithsonian Report for 1916, Publication No. 2457, pp. 239-48.

1918

What American Science Is Doing for Aviation. Paper presented before Northwestern University, Chapter of Sigma Xi, February 4, 1918. Also before Chicago Academy of Science, January 25, 1918, and Western Society of Engineers, February 5, 1918.

1918

Notes on the Accurate Plotting of Aerial Surveys from Photographs. Ms. typed, 10 pp. and 2 plates, for Bureau of Standards. Probably 1918.

1918

Keeping Engineering Education Going at Northwestern University. Paper presented before students at College of Engineering of Northwestern University, October 31, 1918.

1919

What Should Be Done to Develop the Civil Use of Airplanes? Paper presented, University Club of Evanston, Illinois, November 19, 1919. Also Western Society of Engineers. Published Journal, Western Society of Engineers, April 5, 1920.

1919

Review J. L. Hosmer's *Geodesy.* (Manuscript sent to *Science,* December 16, 1919.)

1920

The Future of the Airplane. Paper presented before the Cosmos Club of Washington, D. C., January 5, 1920.

1920

Colleges Transmit Concentrated Experience. Published in *Hexagon* (Alpha Chi Sigma Fraternity) January 1920, pp. 140-144.

1920

The Effect of Topography and Isostatic Compensation Upon the Intensity of Gravity. Special Publication No. 12, Coast and Geodetic Survey, pp. 132 and 28. (Joint author with Wm. Bowie.)

1921

Isostasy. Paper presented Section Geodesy of American Geophysical Union, National Research Council, April 19, 1921. Published by National Academy of Science, Bulletin National Research Council, Vol. 3, Pt. 2, No. 17, 1922.

1922

Effect of Winds and of Barometric Pressures on the Great Lakes. Carnegie Institution of Washington, Publication No. 317, 150 pp.

1923

Recent Progress in Geodesy. Bulletin of the National Research Council, Vol. VII, Part 5, January 1923, No. 41.

1923

Engineering Education at Northwestern University. Bulletin, Northwestern University, 1923. Leaflet, 11 pp.

1923

Engineering, An Occupation Which Arouses Enthusiasm. Radio broadcast paper, WMAQ, Chicago, November 14, 1923.

1924

Engineering as a Vocation. Northwestern University Bulletin, Vol. XXIV, No. 36, March 1, 1924. Leaflet, 10 pp.

1924

The Slides of the Panama Canal. Assisted in preparation of Final Report by the Committee of the National Academy of Sciences. Final report signed by J. F. Hayford, April 2, 1917. Published in Vol. 18, Memoirs, National Academy of Sciences, Washington.

1924

The Establishment of Isostasy. Journal Western Society of Engineers. September 1924, pp. 350-362. This paper won the award of the Chanute Medal for 1924.

1924

The Best Use of the Waters of the Great Lakes. Paper presented before Rotary Club of Evanston, April 17, 1924. *Scientific Monthly*, 1924.

1924

Stream Flow. Paper presented before Washington Society of Engineers, October 15, 1924.

1924

A Method of Estimating Stream Flow and Its Application in the Hydro-Electric Field. (Manuscript Thesis with B. F. Fisher, College of Engineering, Northwestern University.)

1924

An Investigation of a Proposed Control of the Elevations of Lakes Michigan, Huron, and Erie by a Dam with Moveable Parts at the Head of the Niagara River. (Manuscript Thesis with A. B. Simons, College of Engineering, Northwestern University.)

1925

Probability of Floods in Streams in Humid Climates. (Manuscript Thesis with T. B. Stitt, College of Engineering, Northwestern University.)

1925

Fifteen Years' Experience with a Five-Year Engineering Curriculum at Northwestern University. (Co-author with W. H. Burger.) Presented Boulder, Colo., Meeting of Society for Promotion of Engineering Education, June 1924. Journal of Engineering Education, N. S., Vol. XV, No. 7, March, 1925.

1928

The Ellipticity of the Earth Is Not a Proof of a Former Liquid State. Paper presented before Section M, A.A.A.S., Baltimore, December 30, 1908. Published (posthumous) American Journal of Science, Series 5, No. 92, Vol. XVI, August, 1928, pp. 121-25.

1928

It is Not Necessary to Place Geodetic Arcs in Various Latitudes. Paper presented before Section D, A.A.A.S., Baltimore, December 29, 1908. Published (posthumous) American Journal of Science, Series 5, No. 92, Vol. XVI, August 1928, pp. 121-25.

1929

A New Method of Estimating Stream Flow, Based Upon a New Evaporation Formula. Carnegie Institution of Washington, 1929, Publication No. 400, 237 pp. with 22 plates. Note: This was left incomplete at time of J. F. Hayford's death in 1925 and was completed by J. A. Folse.

DATE UNKNOWN

Thickness of Earth's Crust According to Geodetic Evidence. Typed Ms., 2 pp. (No note regarding use. Found among J. F. H. papers.)

The Part Taken by the Philosophical Society in the Development of Geodesy. (Résumé by various authors, including J. F. Hayford, of papers 1877-1899.)

E. J. Wilczynski

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XVI—SIXTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

ERNEST JULIUS WILCZYNSKI

1876–1932

BY

ERNEST P. LANE

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1934

ERNEST JULIUS WILCZYNSKI

November 13, 1876–September 14, 1932

BY ERNEST P. LANE

I. INTRODUCTORY SKETCH

The outstanding features of Wilczynski's life may be sketched briefly as follows.[1] He was born in Hamburg, Germany, on November 13, 1876, the son of Max and Friederike (Hurwitz) Wilczynski; he died in Denver, Colorado, on September 14, 1932. After he had gone to school two years in Hamburg, his family migrated to the United States and settled in Chicago, where his father became a naturalized citizen. The boy Wilczynski attended the elementary schools and the North Division High School of Chicago, completing his high school education in three years.

Then with the assistance of an uncle, Ellis Wilczynski of Hamburg, young Wilczynski returned to Germany for the purpose of entering the University of Berlin. Here he studied for four years under such men as Fuchs, Hensel, Plank, Pringsheim, Schlesinger, Schwarz, and Bauschinger. He received the degrees of A. M. and Ph. D. from the University of Berlin in 1897, being then in his twenty-first year.

Returning to the United States, and failing to secure a position in a university immediately, Wilczynski became temporarily a computer in the Office of the Nautical Almanac at Washington, D. C. It was through the influence of A. O. Leuschner, who had known him at Berlin, that he became instructor in mathematics at the University of California in 1898. There he was successively instructor, 1898-1902; assistant professor, 1902-06; and associate professor, 1906-07. However, he was abroad as research assistant and associate of the Carnegie Institution of Washington for two years, 1903-05. He was mar-

[1] See *Who's Who in America; Annual Register* of the University of Chicago; *Vita* in his Ph. D. thesis.

ried to Contessa Inez Macola of Verona, Italy, on August 9, 1906. She and three daughters survive him.

Wilczynski was associate professor of mathematics at the University of Illinois, 1907-10. Then he was associate professor of mathematics at the University of Chicago, 1910-14; professor, 1914-26; and professor emeritus from 1926 until his death. His health failed gradually after 1919, but he resolutely continued at his post until early in the summer quarter of 1923, when in the midst of a lecture he finally realized that he could go no further and, with a simple statement to that effect, walked from his class-room never to return, leaving his students amazed by the classic self-restraint with which he accepted his tragic fate. It is characteristic of the man, however, that during the nine-years' invalidism which followed he never lost interest in geometry and never gave up hope and the belief that he would some day be able to return to his academic duties.

Wilczynski received during his lifetime several scientific honors and recognitions, of which the most significant are perhaps the following. He was lecturer at the New Haven Colloquium of the American Mathematical Society in 1906, with E. H. Moore and Max Mason. He was at one time vice-president of the American Mathematical Society; he was for two years chairman of the Chicago Section, and served for a period as associate editor of the *Transactions,* of this society. He was also at one time a member of the Council of the Mathematical Association of America. He won a prize of the Royal Belgian Academy of Sciences in 1909, and was chosen a member of the National Academy of Sciences in 1919.

II. CLASSIFICATION OF PUBLICATIONS

We shall analyze Wilczynski's publications and thus seek to arrive at an estimate of his original contributions to science. In this connection we have prepared a bibliography of his publications, which will be found in Section X of this memoir, and which contains a total of seventy-seven titles. This total does not include forty-six abstracts of papers presented to the American Mathematical Society. These abstracts were all published

in the *Bulletin of the American Mathematical Society* and form of themselves an impressive outline of a large part of his work. References to the Bibliography will be made by year and number, as for example (1895. 1).

Wilczynski's seventy-seven publications can be divided into six classes according to the subject matter treated, and to a certain extent chronologically, as follows:

1.	Astronomy and applied mathematics......	15
2.	Differential equations..................	8
3.	Projective differential geometry of curves and ruled surfaces...................	16
4.	Projective differential geometry of surfaces and congruences....................	17
5.	Functions of a complex variable.........	4
6.	Miscellaneous	17

Of course this classification is to a degree arbitrary, but is convenient and will provide a basis for organizing our discussion.

Other classifications would be possible. For instance, there might be twenty classes according to place of publication, since Wilczynski published papers in nineteen periodicals and also published certain works privately. Among the journals, he favored the *Transactions of the American Mathematical Society,* the *Bulletin* of this Society, and the *American Journal of Mathematics.* Still another classification would be according to language. Wilczynski published works in German, French, and Italian, besides English.

III. ASTRONOMY AND APPLIED MATHEMATICS [2]

Wilczynski began his scientific career as a mathematical astronomer, his first published paper (1895. 1) being an exposition in English of Schmidt's theory of the sun. It seems appropriate that Wilczynski, who in later years came to be known as a master of the difficult art of elegant mathematical exposition, should have devoted his first effort to exposition. This

[2] I am indebted to my friend and former colleague, Professor L. La Paz, now of Ohio State University, who kindly collaborated with me in studying Wilczynski's work in astronomy and applied mathematics.

first paper was one of five (1895. 1. 2), (1896. 1. 2), (1897. 1) written while Wilczynski was still a student at the University of Berlin and published before his thesis. Its aim was to present to American astronomers an account of Schmidt's theory,[3] which was gaining some attention[4] in Germany. This theory adopted the methods of geometrical optics and attempted to account for many solar phenomena by means of the laws of refraction. But in spite of Wilczynski's exploitation and promulgation, Schmidt's theory never gained great favor with observational astronomers. In fact, Wilczynski's second paper (1895. 2) consists of extracts from two letters from him to G. E. Hale, in which Wilczynski undertook to defend Schmidt's theory by answering several objections to it that had been raised. But Hale concluded the argument with the statement: "As a theoretical discussion the theory is interesting and valuable, but few observers of the sun will consider it capable of accounting for the varied phenomena encountered in their investigations."

After dropping Schmidt's theory, Wilczynski began to publish original researches of his own. He produced a series of ten papers which appeared from 1896 to 1899, and which are all rather closely connected with his thesis (1897. 2) entitled "Hydrodynamische Untersuchungen mit Anwendung auf die Theorie der Sonnenrotation." Wilczynski's first paper on hydrodynamical investigations of the solar rotation (1896. 1) contained an error in a proof, which was pointed out by Harzer.[5] Wilczynski replied to Harzer's criticisms in a brief note (1897. 1). Wilczynski while still a student also published (1896. 2) a theory of spiral and planetary nebulae.

It will not be necessary to report here in detail on the contents of each paper of the series on the solar rotation. For the main features of the theory the reader may consult two of the

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[3] August Schmidt, "Die Strahlenbrechung auf der Sonne; ein geometriches Beitrag zur Sonnenphysik," Stuttgart (1891).

[4] O. Knopf, "Die Schmidt'sche Sonnentheorie und ihre Anwendung auf die Methode der spektroskopischen Bestimmung der Rotationsdauer der Sonne," Habilitationsschrift, Jena (1893).

[5] Harzer, *Astronomische Nachrichten,* No. 3386.

papers besides the thesis, namely, a popular account (1898. 2) and a more mathematical account (1897. 3). The following remarks will give some idea of their contents. Wilczynski supposed that the sun is a viscous fluid, the particles of which attract each other according to the Newtonian law and describe circles, lying in parallel planes, about an axis perpendicular to these planes. The temperature and density of the fluid, and the angular velocity of rotation of its particles, were regarded as functions of position. The problem then was to determine the character of the motion and the form of the mass. Wilczynski showed that *if the surfaces of constant density coincide with the surfaces of constant pressure, the angular velocity of rotation is the same for all particles equally distant from the axis of rotation. Moreover, the velocity is an increasing function of the distance.* Thus Wilczynski gave an explanation, afterward regarded by F. R. Moulton as fairly satisfactory,[6] of the interesting fact that the angular velocity of rotation of the sun decreases from the equator to the poles. Wilsing and Harzer had reached a similar explanation somewhat earlier, but Wilczynski's explanation was independent of theirs, and was more rigorously deduced.

Wilczynski believed that on the basis of his theory he could explain the sun-spot period (1896. 2), (1898. 3), the differences in level of the faculae and the spots, and the depth of the reversing layer (1898. 4), as well as certain peculiarities of nebulae (1896. 2), (1899. 4), and the variable velocities of the spots on Jupiter (1898. 1). Especially in the light of recent astronomical discoveries, we now may be justified in not exhibiting for these latter explanations the same enthusiasm which their author showed for them at the time when they were written.

The third and last sub-group of papers on astronomy and applied mathematics consists of three rather disconnected publications. The first of these (1900. 3) is interesting because it is the first of the series of twenty-two papers that Wilczynski published in the *Transactions of the American Mathematical*

[6] Moulton, *Introduction to Astronomy*, MacMillan, 1926, p. 390.

Society, and also because it is one of the earliest manifestations of the deep influence that the Lie theory of continuous groups had upon Wilczynski after he had been introduced to this theory by L. E. Dickson then at the University of California. In this paper Wilczynski followed up an observation of Lie to the effect that the steady motion of a fluid is an image of a one-parameter group in three variables. He specialized the group in various ways and discussed the corresponding motion.

In the second paper (1912. 1) of this sub-group, Wilczynski called attention to a theorem, which he had discovered independently and later found in Newton's *Principia,* to the effect that *if the mean anomaly* M *and the radius vector* r *of a planet are considered as rectangular coordinates, and if the units of measure are suitably chosen, the* (M, r)*-curve is a trochoid.* He presented with M. J. Eichorn the design of an instrument based on this theorem for the mechanical solution of Kepler's equation.

The last paper (1913. 1) of this series is interesting, for one reason, because it is the only paper that Wilczynski ever published in Italian. He and Dickson had studied this language together for a year at the University of California prior to Wilczynski's appointment by the Carnegie Institution. Under this appointment Wilczynski had studied at Rome, as well as at Göttigen, Paris, and Cambridge. Italian was the native language of his wife. He and his students published rather frequently in Italian journals, but only this paper (1913. 1) in the Italian language. In it Wilczynski made use of some of the methods of projective differential geometry to study certain curves and ruled surfaces that occur in the problem of three bodies, namely, the curves which are the loci of the three bodies when their center of gravity is at rest, the ruled surfaces generated by the straight lines joining pairs of the bodies, and the cone enveloped by the plane of the bodies.

IV. DIFFERENTIAL EQUATIONS

In his work in theoretical astronomy Wilczynski had occasion to use differential equations, and one may say that the subject of differential equations served as a bridge over which his in-

terest crossed the gap between astronomy and geometry. It is naturally not any too easy to say which of his papers should be classified as being primarily memoirs on differential equations, since nearly all of his work has to do with differential equations in one way or another.

The first papers which Wilczynski published on other subjects than those associated with astronomy and applied mathematics appeared in 1899. In this year he published four papers on differential equations. One (1899. 1) of these bears the title, "On an mn^2 parameter group of linear substitutions in mn variables." The title would indicate that this paper is largely concerned with Lie's theory of continuous groups, and so it is. In fact, much of Wilczynski's work was dominated by this concept. But a brief analysis of this paper would show that it is fundamentally concerned with differential equations.

A second paper (1899. 6) bears the title, "A generalization of Appell's factorial functions," and is interesting, for one reason, because it is the first paper that Wilczynski published in the *Bulletin of the American Mathematical Society.* The subject would indicate that this paper is in a sense a contribution to the theory of functions, and so it is. But Wilczynski insisted that the functions considered can appear as integrals of differential equations. This paper is connected with three other papers (1899. 2. 3), (1900. 2) which appeared in the *American Journal of Mathematics.* The first (1899. 2) of these may be explained as follows. The integrals of a linear differential equation with uniform coefficients have the property that they are uniform and continuous everywhere except in the vicinity of the singular points of the equation, where they undergo, in general, linear substitutions with constant coefficients. Fuchs took the differential equation as given, and his problem was to determine the substitution group belonging to the integrals. Riemann took the converse problem and supposed that the branch points and fundamental substitutions were given; and the question was on the existence of a system of functions having the given substitutions and branch points. Riemann, and also Klein, proved existence theorems in special cases. Wilczynski

did not solve the problem completely, but proved the existence of a large class of functions by a method which consists in generalizing the hypergeometric functions. In another paper (1899. 3) Wilczynski observed that the fundamental notions of the theory of linear differential equations can be applied to a large class of non-linear differential equations, called by him *linearoid* differential equations. In the last paper (1900. 2) of this sequence, Wilczynski specialized the situation of the preceding paper and studied the groups, the differential equations, and their solutions in some detail in the special case in which the number of dependent variables is 2 instead of n.

The next two papers of this group are especially significant. The first (1901. 1) of these may be said to mark the beginning of Wilczynski's career as a geometer, not because of any purely geometrical results that it contains, of which there is none, but rather because the material in it was later used as the content of Chapter I of his book (1906. 1) on the projective differential geometry of curves and ruled surfaces, which established his reputation as a geometer. In this paper Wilczynski determined the most general transformation that leaves invariant the form of a system of n independent linear homogeneous differential equations of order at most m in n dependent variables and one independent variable. The second paper (1901. 2) was later included in Chapter IV of the book. In this paper the system of equations

$$\begin{aligned} y'' + p_{11}y' + p_{12}z' + q_{11}y + q_{12}z &= 0, \\ z'' + p_{21}y' + p_{22}z' + q_{21}y + q_{22}z &= 0, \end{aligned}$$

which is fundamental in Wilczynski's theory of ruled surfaces appeared for the first time, this being the system to which that of the preceding paper reduces in the special case $m = n = 2$. The paper is mostly taken up with the calculation of seminvariants and invariants of this system of equations under the most general transformation that leaves the form of the system invariant, by means of Lie's theory of continuous groups.

Wilczynski's last paper (1914. 4) primarily on the subject of

differential equations appeared much later, and will be mentioned again in Section VI in connection with a paper (1913. 2) on the geometry of surfaces.

V. PROJECTIVE DIFFERENTIAL GEOMETRY OF CURVES AND RULED SURFACES

When Wilczynski published in book form (1906. 1) his theory of the projective differential geometry of curves and ruled surfaces, he gave to the world a new method in geometry, and established himself as the leader of a new school of geometers, which may be called the American school of projective differential geometers. His influence, moreover, was international and was particularly strong in Italy and Czechoslovakia.

A few general remarks about Wilczynski's method seem to be in order here. Let us suppose that we have before us a configuration whose projective differential geometry is to be studied. Let us write the parametric equations of this configuration by expressing the projective homogeneous coördinates of a general element of the configuration as functions of a certain number of parameters. Then let us calculate the coefficients of a completely integrable system of linear homogeneous differential equations of which these coordinates constitute a fundamental set of solutions. To say that the system of equations is *completely integrable* means that the most general solution can be expressed as a linear combination of a fundamental set of solutions with constant coefficients. Speaking of the original configuration as an *integral configuration* of the system of equations, we can show that the most general projective transform of the configuration is also an integral configuration of the system of equations, and that every integral configuration can be obtained in this way. Consequently *a geometric theory based on the differential equations is a projective theory.* The next step is to determine the most general transformation of dependent and independent variables that leaves the configuration invariant. The effect of this transformation on the differential equations is then calculated. A combination of the coefficients

of the differential equations, and their derivatives, which is changed by the transformation at most to the extent of being multiplied by a factor which depends only on the transformation is called an *invariant*. *Absolutely invariant equations connecting such invariants express projective properties of the integral configurations*. Furthermore, a combination not only of the coefficients and their derivatives but also of the dependent variables and their derivatives which is similarly invariant under the transformation is called a *covariant*. *Every covariant defines a configuration which has its elements in correspondence with the elements of the original configuration, and which can be constructed from the original configuration by means of a projective geometric construction*. Wilczynski's method, in brief, consists in studying configurations by means of these invariants and covariants.

Since references to Wilczynski's work on curves and ruled surfaces are usually made to the book of 1906, not every one knows that, at that date, a very large portion of this book had already been published in the form of memoirs. In Section IV we discussed two of these memoirs (1901. 1. 2). Besides these two there were published from 1901 to 1905 in the *Transactions of the American Mathematical Society* eight memoirs which were later included in the book, and which we now proceed to discuss.

In the first (1901. 3) of these eight memoirs Wilczynski introduced the idea of an *integral ruled surface* of the system of two ordinary differential equations of the second order whose invariants he had previously calculated (1901. 2). He calculated the linear homogeneous differential equation of the sixth order satisfied by the coordinates of a generating line of a ruled surface, and deduced the conditions that an integral ruled surface belong to a linear complex, to a special linear complex, or to a linear congruence, as well as the conditions for being a quadric surface. He studied in some detail the asymptotic curves on a ruled surface.

In the next paper (1902. 2) Wilczynski applied the principle of duality to some of his previous considerations, and calculated

the adjoint system of differential equations, showing that the two systems coincide in case the integral ruled surfaces are quadrics.

In the third paper (1902. 3) Wilczynski developed a theory of covariants, and considered the *osculating quadric* along a generator of a ruled surface, namely, the quadric determined by the generator and two consecutive generators. He considered the *flecnodes* on a generator, which are defined to be the two points where four-point tangents, called flecnode tangents, can be drawn; the locus of the flecnodes is by definition the *flecnode curves* on a ruled surface. He also introduced the *flecnode congruence* of a ruled surface; this consists of the generators of the osculating quadrics that are not asymptotic tangents.

In the fourth paper (1903. 2) Wilczynski studied the flecnode congruence more thoroughly, determining its developables and focal surfaces. He considered also the *flecnode surfaces,* which are by definition the focal surfaces of the flecnode congruence, and are also the loci of the *flecnode, or four-point, tangents* of the original ruled surface. He further announced the theorem which is fundamental for the so-called *flecnode transformation* of ruled surfaces, and which states that *the original surface is a flecnode surface of each of its flecnode surfaces.*

In the fifth paper (1904. 3) Wilczynski proved that *the flecnode congruence of a ruled surface is a* W *congruence,* i. e., that the asymptotic curves correspond on its two focal surfaces. He introduced the *principal ruled surface* of the flecnode congruence, which is a well-defined ruled surface covariant to the original surface. He also considered the *osculating, or five-line, linear complex* along a generator of a ruled surface, and the null system of this complex.

In the sixth paper (1904. 4) Wilczynski took up the case, previously excluded, of ruled surfaces whose flecnode curves coincide. In the seventh paper (1905. 1) he studied the general theory of curves on ruled surfaces.

Finally, in the eighth paper (1905. 2) Wilczynski studied curves in ordinary space. Many of the osculants used had been previously introduced by G. H. Halphen (1844-89), who was

probably the first ever consciously to undertake and carry to fruition a projective differential investigation. The *osculating conic* is perhaps Wilczynski's most important geometrical contribution to the theory of space curves. This conic, at a point of a curve, is defined to be the conic cut on the osculating plane of the curve by the tangent developable of the osculating twisted cubic of the curve at the point.

Besides the series of eight memoirs which we have just been discussing there was a paper (1904. 1) in the *Mathematische Annalen* in which a theorem on "self-dual" ruled surfaces was announced, to the effect that a self-dual ruled surface must belong to a linear complex. But the theorem was later admitted to be "badly formulated," and so there was a note (1904. 2) in the *Bulletin of the American Mathematical Society* correcting the error, by saying that the dualistic correspondence must be such that it converts each generator of the ruled surface into itself before the theorem is true.

Wilczynski presented some of his results to the Third International Mathematical Congress at Heidelberg, August 8-13, 1904, and his paper (1905. 3) was published in the proceedings of this congress. He was also lecturer at the New Haven Mathematical Colloquium in 1906, as has already been mentioned, his lectures being published later (1910. 1).

On December 30, 1915, at Columbus, Ohio, Wilczynski gave an address as retiring chairman of the Chicago Section of the American Mathematical Society. In this address he outlined the theory of a single plane curve, and in particular presented some metric results which had been found by A. Transon before Wilczynski rediscovered them independently. These are concerned with the axis of aberrancy, or affine normal, at a point of a curve, the osculating parabola, the ellipse of minimum eccentricity among the four-point conics, and other matters.

In a paper (1916. 3) in the *Proceedings of the National Academy of Sciences* Wilczynski gave a geometric interpretation of a simple integral invariant associated with a given plane curve, which has since been called by Sannia the *projective arc length* of the curve, and in another paper (1917. 1) he elaborated

the details of this interpretation, which uses an *infinite product* of cross ratios, instead of an *infinite sum* of terms as in an ordinary definite integral.

VI. PROJECTIVE DIFFERENTIAL GEOMETRY OF SURFACES AND CONGRUENCES

It is natural that surfaces and congruences should be studied together, because a surface is a *two-parameter* family of points and a congruence is a *two-parameter* family of straight lines. Consequently the projective differential geometry of each of these configurations is studied, according to Wilczynski's method, by means of *partial* differential equations in two independent variables.

There are five papers that are concerned more or less primarily with congruences. The first (1904. 5) of these marks the transition of Wilczynski's interest from the ordinary differential equations of curves and ruled surfaces to the partial differential equations of surfaces and congruences. Its contents may be outlined briefly as follows. Wilczynski began by determining the most general transformation that preserves the form of a system of q partial differential equations of the first order in n dependent variables and m independent variables. He then confined his attention to the case $q = n = m = 2$, and calculated the invariants and covariants in this case. He next introduced the geometry of a congruence and connected this geometry with the special system of equations. For an integral congruence of the equations he determined the developables and focal surfaces. It may be remarked that in this paper Wilczynski used a system composed of only two first-order equations; although he was thinking of a congruence in ordinary space, he did not introduce the two equations of the second order which, with the two equations of the first-order, form a completely integrable system for the theory of congruences in ordinary space. It may be remarked further that Wilczynski's approach to a geometrical problem here, as usually elsewhere, was through the analysis. He ordinarily preferred to start with a system of equations, to determine the appropriate transforma-

tions, to compute the invariants and covariants, and finally to interpret his results geometrically, instead of starting with the geometrical problem itself, guiding himself by geometrical intuition, and relegating the analysis to the subordinate place of a tool.

The second (1911. 2) of the five papers on congruences is the so-called "Brussels paper" entitled "Sur la théorie générale des congruences," which won a prize of the Belgian Academy of Sciences in 1909. In this paper appeared for the first time the completely integrable system composed of two first-order and two second-order equations which characterizes a congruence in ordinary space, except for a projective transformation, and so the theory of such congruences was established on a solid foundation. The developables of the congruence under consideration in the Brussels paper were taken as the parametric ruled surfaces of the congruence. Invariants and covariants were computed. The completely integrable system of two second-order differential equations of each focal surface of the congruence were calculated, as well as the fourth-order differential equations of the parametric curves on these surfaces. Two special types of congruences were studied in some detail; namely, congruences belonging to linear complexes and having Laplace transforms likewise belonging to linear complexes, and congruences whose focal surfaces are quadrics.

The third paper (1915. 1) on congruences is intimately connected also with the theory of surfaces. In this memoir Wilczynski introduced the *axis congruence* and *ray congruence* associated with a conjugate net on a surface in ordinary space, the *axis* of a point on the surface being defined to be the line of intersection of the osculating planes of the two curves of the net at the point, and the *ray* being defined as the line joining the corresponding points, called *ray-points,* on the edges of regression of the two developables circumscribing the surface along these curves. Wilczynski pointed out the dualistic relation between these two congruences, and showed that *a conjugate net has equal Laplace-Darboux invariants if, and only if, the ray curves, i. e., the curves which correspond on the surface to the developables of the ray congruence, themselves form*

a conjugate net. Moreover, he showed that the fundamental conjugate net has the property of being *isothermally conjugate* in case a certain fairly simple algebraic relation exists between three invariants which had already been interpreted geometrically, thus in a sense giving a geometric significance to the property of isothermal conjugacy. The fourth paper (1915. 2) is an account of these results in the *Proceedings of the National Academy of Sciences.*

Finally, in the fifth paper (1920. 2) we have a theory of congruences in which a congruence is regarded at first as a one-parameter family of ruled surfaces, and then as a net of ruled surfaces. In this paper Wilczynski introduced the idea of a *conjugate net of ruled surfaces in a congruence,* which is merely a net of ruled surfaces such that at each point of each generator of the congruence the tangent planes of the two ruled surfaces through the generator separate harmonically the focal planes of the generator.

Let us now turn our attention to the twelve papers that contain Wilczynski's work on the theory of surfaces. His interest in surfaces was confined for the most part to surfaces in ordinary space. The foundations of his theory of the projective differential geometry of surfaces in ordinary space were laid in a series of five memoirs published in the *Transactions of the American Mathematical Society* from 1907 to 1909.

In the First Memoir (1907. 1) Wilczynski reduced the defining pair of linear homogeneous partial differential equations of the second order for a non-developable surface to the so-called *intermediate form,*

$$y_{uu} + 2ay_u + 2by_v + cy = 0,$$
$$y_{vv} + 2a'y_u + 2b'y_v + c'y = 0,$$

by taking the asymptotic curves on an integral surface as parametric. He calculated the integrability conditions for this system, as well as a complete system of invariants and covariants. He further reduced the system of equations to a *canonical form,* characterized by the conditions $a = b' = 0$, and determined the most general transformation leaving this form invariant. He

arrived at the adjoint system of equations of the intermediate system, wrote conditions necessary and sufficient that the integral surfaces be ruled, namely $a' = b = 0$, and proved a so-called *fundamental theorem* to the general effect that a surface is determined except for a projective transformation by giving four functions of two variables, subject to certain integrability conditions.

In the Second Memoir (1908. 1) Wilczynski did some of his best work. He derived the local equation of the *quadric of Lie,* called in this memoir the *osculating quadric,* at a point of a surface. He studied the *osculating linear complexes* of the two asymptotic curves at a point of a surface, and also the osculating linear complexes along the generators through the point of the two skew ruled surfaces of asymptotic tangents which circumscribe the surface along the asymptotic curves through the point. He introduecd the two *directrices* at a point of a surface as the directrices of the linear congruence of intersection of the two osculating linear complexes of the asymptotic curves at the point, and studied the two *directrix congruences* composed of the directrices for all points of the surface. He proved that the developables of both congruences correspond to the same net of curves, called the *directrix curves,* on the surface. He calculated to a limited number of terms a *canonical power series expansion* for one non-homogeneous projective coordinate of a point on a surface in terms of the other two coordinates. This series for a non-ruled surface is of the form

$$z = xy + (x^3 + y^3)/6 + (Ix^4 + Jy^4)/24 + \ldots,$$

where I, J are certain absolute invariants of the surface. In defining geometrically the local coordinate system for this expansion, Wilczynski introduced the *canonical cubic* and *canonical quadric*. The *canonical cubic* at a point of a surface he defined by the following properties. *It has a unode on the directrix through the point, such that the uniplane contains the directrix that lies in the tangent plane of the surface at the point. It, further, has third-order contact with the surface at the point. Finally, the four tangents of fourth-order contact form a har-*

monic set in which conjugate pairs are actually conjugate tangents of the surface. The *canonical quadric* at the point of the surface he then defined by the following properties. *It has second-order contact with the surface at the point. It, further, is tangent to the quadric of Lie at all points of the two generators through the point. Finally, it is tangent to the uniplane of the canonical cubic.* G. M. Green later pointed out that the first property of the quadric just mentioned is superfluous, being a consequence of the second property. We now-a-days restate the essential part of this definition by saying that *the canonical quadric is the quadric of Darboux that is tangent to the uniplane of the canonical cubic.* Green pointed out, further, that it was evidently desirable to have a characterization of the canonical quadric which is independent of the canonical cubic. Such a characterization was later furnished by E. Bompiani [7] in 1927, and another by E. B. Stouffer [8] in 1932.

In the Third Memoir (1908. 2) Wilczynski applied to ruled surfaces some of the considerations of the Second Memoir, especially the power series expansion and the geometric description of the associated local coordinate system. In the Fourth Memoir (1909. 3) he laboriously calculated the integrability conditions and invariants for the defining pair of equations of a general analytic surface in ordinary space without specializing the parameters.

Finally, in the Fifth Memoir (1909. 4) Wilczynski gave by way of introduction a noteworthy historical preface on the relation of this memoir to some work of Moutard, Darboux, and Segre, part of which he had done again independently. He studied here especially the *tangents of Darboux,* the *quadric of Moutard,* and the *osculating Steiner surface* at a point of an analytic surface.

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[7] For further discussion see Lane, *Projective Differential Geometry of Curves and Surfaces,* University of Chicago Press, 1932, p. 80 and p. 295.

[8] Stouffer, "A geometrical determination of the canonical quadric of Wilczynski," *Proceedings of the National Academy of Sciences,* vol. 18 (1932), pp. 252-5.

In a subsequent paper (1913. 2) Wilczynski studied those surfaces for which the absolute invariants I, J vanish identically. Wilczynski characterized these surfaces by the property that *at each point of one of them the canonical cubic has contact of the fourth order with the surface.* These surfaces can also be characterized, in more recent terminology, by the property that at each point of one of them the canonical lines of the first kind all coincide, as do also all the canonical lines of the second kind. For this reason, these surfaces are now commonly called *coincidence surfaces.* They are integral surfaces of a pair of equations which can be reduced to the form

$$y_{uu} + 2y_v + (c_0u + c_1)y = 0,$$
$$y_{vv} + 2y_u + (c_0v + c_2)y = 0 \qquad (c_0,\ c_1,\ c_2 \text{ consts.}).$$

Wilczynski integrated this system of equations quite simply when $c_0 = 0$, and studied the integral surfaces in this case, showing, among other things, that *in general such an integral surface is invariant under a two-parameter group of projective transformations.* In another paper (1914. 4), to which reference was made at the close of Section IV, Wilczynski integrated the equations when $c_0 \neq 0$ by four independent power series.

In one of his papers (1911. 1) Wilczynski studied *nets of curves in the plane* by means of the invariants and covariants of a system of three equations of the second order, noticing particularly the osculating conics of the two curves of the net at a general point thereof. In a later paper (1914. 3) he studied *surfaces on which the directrix curves are indeterminate.* For such a surface the directrices of one kind form a bundle of lines, and those of the other kind form a ruled plane. The center of the bundle is not ordinarily on the plane, and, when it is not, the projection of the asymptotic curves on the surface from the center of the bundle onto the ruled plane is a *plane net of period three with equal Laplace-Darboux invariants.* If the point is the origin and the plane is the plane at infinity, the surface with indeterminate directrix curves belongs to a class of surfaces which have been studied by G. Tzitzéica under the group of affine transformations leaving the origin invariant.

There are two papers which are primarily concerned with the property of *isothermal conjugacy* of a net of curves on a surface in ordinary space. Bianchi had defined the property and shown that it was of a projective nature without finding its geometric significance. We have already seen that Wilczynski was interested in the problem of finding the geometric significance of the property in another paper (1915. 1). In 1916 G. M. Green gave a descriptive property of isothermally conjugate nets and thought he had solved the problem completely, but he had overlooked an exceptional case in which the property that he gave fails to distinguish between isothermally conjugate nets and a kind of conjugate nets now commonly called, with Wilcznyski, *harmonic conjugate nets.* The first (1920. 1) of Wilczynski's papers with which we are concerned here was intended to complete the work of Green on this subject. In this paper Wilczynski solved the problem completely, using the notion of a *pencil of conjugate nets.* This notion has proved to be a very fruitful one in the theory of conjugate nets. In a second paper (1922. 1) Wilczynski recounted the history of the problem and stated the results of the first paper. Moreover, he gave an account of further developments in this direction, as he had discovered in the meantime a still more elegant characterization of the property of isothermal conjugacy, to the following effect. A conjugate net of curves on a surface in ordinary space determines a pencil of conjugate nets. As a general net of this pencil varies over the pencil, the ray-points corresponding to a point of the surface describe a nodal cubic curve lying in the tangent plane of the surface at the point. The three inflexions of this cubic lie on a straight line called the *flex-ray of the point.* For all points on the surface these flex-rays form a congruence. The developables of this congruence correspond to a conjugate net of curves on the surface if, and only if, the fundamental net is isothermally conjugate.

In what seems to have been the last paper that Wilczynski wrote (1922. 2), he studied *hypergeodesics* on a surface, i. e., curves defined by a curvilinear differential equation of the form

$$v'' = A + Bv' + Cv'^2 + Dv'^3,$$

in which $v' = dv/du$, and the coefficients are functions of u, v. He pointed out the relation between what is called *Segre's correspondence* and the so-called *polarity of Lie,* and gave an interpretation of *Fubini's integral invariant* $\int a'b\, du\, dv$ after the manner of his interpretation of the simplest integral invariant of a plane curve.

In the meantime Wilczynski had delivered an address in Cleveland, Ohio, on December 31, 1912, before a meeting of some of the sections of the American Association for the Advancement of Science. This address (1913. 3) was published under the title "Some general aspects of modern geometry." Later it was translated into Italian by Bompiani and republished [9] under the title "Alcune vedute generali della geometria moderna." This address gives what is perhaps Wilczynski's best account of the general aspects of his method.

VII. FUNCTIONS OF A COMPLEX VARIABLE

In the later years of his activity Wilczynski seemed to be turning more and more toward the domain of functions of a complex variable. It is vain to speculate on such questions as how far in this direction he would have gone had his health permitted, or what his reaction would have been to the modern enthusiasm for non-riemannian geometry. But we may feel sure that he would have made significant contributions to all subjects in which he became interested, and such was the case, certainly, in the domain of functions of a complex variable, although we have from his pen only four papers on this subject.

The first two of these papers are related to each other. In the first paper (1919. 2) Wilczynski studied two line-geometric representation of a function of a complex variable. According to the first method one plots an independent complex variable in a plane and in a plane parallel to this plane a complex function of this variable. Each point representing a value of the independent variable is joined by a straight line to the point repre-

[9] *Bollettino di biographia e storia delle scienze mathematische,* vol. 16 (1914), p. 97.

senting the corresponding value of the function. Thus a congruence of lines is obtained, which is a geometric representation of the function. According to the second method of exhibiting the functional relation geometrically, one first plots the independent and dependent variables in the same plane, which is taken as the plane $z = 0$ of a three-dimensional orthogonal cartesian coordinate system. The points representing these variables are next projected stereographically from the point $(0, 0, 1)$ onto a sphere with unit radius and with its center at the origin. Corresponding points on this sphere are then joined by straight lines, which form a congruence. So, by two methods, the theory of rectilinear congruences is connected with the theory of functions of a complex variable. In the second paper (1920. 3) Wilczynski showed that a certain set of seven properties is characteristic of those congruences each of which can represent a function of a complex variable by the method just described of joining points on a sphere.

The last two papers (1922. 3), (1923. 1) are really two parts of the same long memoir. Although the second part of this memoir bears the latest date of publication of any of Wilczynski's papers, it was complete, or practically so, as early as 1918, publication being delayed probably by the war. In this memoir on *projective differential properties of a function of a complex variable* Wilczynski first considered a function w of a complex variable z and subjected z to a linear fractional transformation to see what properties of the function w remained invariant under this transformation. Some of these properties are uniformity, the possession of singular points and the cross ratio of four singular points or four zeros. Wilczynski calculated both differential and integral invariants and wrote an *intrinsic equation* of the function, besides introducing what he called the *osculating logarithm* at a point of the function. Further on Wilczynski considered independent linear fractional transformations of both dependent and independent variables, and calculated what he called *hyperinvariants* and a *hyperintrinsic equation* of the function.

VIII. MISCELLANEOUS

The versatility of Wilczynski's mind has already been amply illustrated, but a glance at his miscellaneous publications will throw a still clearer light on the diversity of his intellectual interests. Two papers especially are perhaps rather surprising. The first (1900. 1) of these appeared in the *University Chronicle* of the University of California, and is entitled "Poetry and mathematics." In this Wilczynski defended the thesis that a poet and a mathematician have certain intellectual and aesthetic elements in common. The second (1909. 2) of these two papers is a popular and philosophical account of "The fourth dimension," which appeared in the *American Mathematical Monthly*.

Two other papers are a result of Wilczynski's active interest in the affairs of the American Mathematical Society. He wrote for the *Bulletin* of this society an account (1902. 4) of the first meeting of the San Francisco Section, giving a report of the organization of the section and of its first program. The second paper (1909. 1) is entitled "Mathematical appointments in colleges and universities." This is a minority report of a committee of the Chicago Section charged with the duty of investigating the possibility of improving the character of mathematical appointments in American colleges and universities.

Another type of Wilczynski's activity consisted in writing book reviews. Of these there seem to have been seven (1898. 5), (1899. 5), (1903. 1), (1909. 5), (1910. 2), (1914. 2. 5). The first two are short reviews of German books on the magnetism of the earth, and appeared in *Terrestrial Magnetism*. The remaining five appeared in the *Bulletin of the American Mathematical Society*. Two of these are brief; the other three are rather pretentious, namely, a review (1903. 1) of part of Forsyth's "Differential Equations," a review (1910. 2) of Schlesinger's "Vorlesungen über lineare Differentialgleichungen," and a review (1914. 2) of Darboux's "Leçons sur les systèmes orthogonaux et les coordonées curvilignes," second edition, complete.

Wilczynski wrote two college textbooks in mathematics, one (1914. 1) a trigonometry, and the other (1916. 1) a college

algebra. In these Wilczynski's powers of elegant mathematical exposition found ample scope, and especially in the trigonometry his fondness for the heuristic method of presenting a subject is fully illustrated.

In a paper (1918. 1), showing the effect of contact with General Analysis, Wilczynski furnished a proof of the fact that *the coefficients of a unique canonical form are invariants,* making use only of the abstract principles which are common to all known invariant theories. In another paper (1919. 1) he established the scale of relation for the coefficients of the power series expansion of an algebraic function as a scale of degree n if the equation defining the function is irreducible and of degree n.

The last two papers in this category are commemorations. The first (1902. 1) of these is a commemoration of one of Wilczynski's former teachers, Lazarus Fuchs. This contains an appreciation of the scientific work of Fuchs but there is no bibliography accompanying it. The second (1919. 3) is a commemoration of one of the most distinguished of American projective differential geometers, G. M. Green of Harvard University, whose already brilliant career was cut short by death at the untimely age of twenty-seven. This paper contains a beautifully written appreciation of the life, character, and work of Green, for whom Wilczynski had the highest regard. It is interesting to note that although the two men were working in the same field, and had carried on a most friendly correspondence, they had never met each other personally. The bibliography of Green's publications prepared by Wilczynski to accompany this commemoration seems to be complete except for one paper published[10] posthumously, after the commemoration was written, by Wilczynski as Green's scientific executor.

IX. CONCLUDING COMMENTS

It is quite likely that Wilczynski had worked on other subjects than those discussed here without publishing his results, which

[10] Green, "Nets of space curves," *Transactions of the American Mathematical Society,* vol. 21 (1920, pp. 207-36).

may not have reached the perfection that he desired. In fact, there is an abstract in the *Bulletin of the American Mathematical Society* of a paper on integral equations which seems never to have been published. There is also an abstract of a paper presented to the Society by title at the meeting in Toronto in December, 1921, on "Surfaces representing the real and imaginary parts of a function of a complex variable," which seems never to have been published in full.

One of the most important of Wilczynski's activities consisted in directing thesis work. There seems to be no record of the number of master's theses that he directed, but there were many of them. Three of them, at least, were published in journals, for references to which the reader may consult the Bibliography which follows. Wilczynski directed twenty-five doctoral dissertations. Two of these were the theses of E. B. Stouffer and of W. W. Denton who took their degrees at the University of Illinois. The other twenty-three were the theses of Chicago doctors. Two of these men, H. L. Olson and J. W. Hedley, started under Wilczynski but the final approval of their work was granted by A. F. Carpenter and E. P. Lane. Wilczynski started the thesis work of two other men, P. G. Robinson and V. G. Grove, not counted here, who made some progress under his direction, but those theses were examined and finally passed upon by Lane. Of the twenty-five doctoral dissertations, nineteen were published in journals and six privately. These theses represent in a sense Wilczynski's own scientific activity, and these students of Wilczynski form an influential group of the American mathematical community. For further information about them the reader may consult the Bibliography.

Wilczynski has already been referred to as a master of exposition. He possessed a most elegant style both in spoken and written English. German was his native language, and he was at home with French and Italian. He was such a clear and polished lecturer that he made difficulties seem easy. If some unforeseen problem arose in class, the solution of which might involve delays and hesitation, it was his custom not to attack the obstacle at once but to defer consideration of it until the next

day when he habitually reported upon the solution that he had reached in the meantime.

Wilczynski's character was gentle; his manner, mild. He was unselfish and was habitually thoughtful of others. He was interested in his students and in their success. He enjoyed dropping in unannounced at the modest lodgings of his advanced graduate students, especially in the spring and summer evenings, to spend an hour or two in informal conversation. It was Wilczynski's art upon such occasions to put every one at ease, and the memories of these friendly visits are cherished in the homes of more than one of his doctors.

Wilczynski was an inspiring teacher, a thorough scholar, a distinguished geometer, a congenial colleague. The premature termination of his scientific career was a great loss to mathematics. When Wilczynski spoke in commemoration of Green he used words which can now appropriately be quoted and employed in commemoration of their author:

> "In this brief span of years he has won enduring fame. . . ., we mourn in him not the promise of a genius unfulfilled, but the sad untimely loss of a great leader of proven strength whose power and insight had been fully tested, and whose actual achievements can never perish. . . . In his death we have suffered a heavy loss, but his life and work will continue to be, for many of us, an everlasting source of strength and inspiration."

X. BIBLIOGRAPHY

The following bibliography includes Wilczynski's publications exclusive of *abstracts* of papers presented to the American Mathematical Society. A list of his doctors is appended, giving the subjects of their theses and the positions held by them in 1934. References are given at the end to three published master's theses.

The following abbreviations taken from *Bulletin* 63 of the National Research Council will be used:

1. Am. J.: *American Journal of Mathematics.*
2. Am. M. S. Bull.: *Bulletin of the American Mathematical Society.*

3. Am. M. S. Trans.: *Transactions of the American Mathematical Society.*
4. Ann. di Mat.: *Annali di Matematica Pura ed Applicata.*
5. Giorn. di Mat.: *Giornale di Matematiche.*
6. Math. Ann.: *Mathematische Annalen.*
7. Pal. Circ. Mat.: *Rendiconti del Circolo Matematico di Palermo.*
8. Tôhoku M. J.: *Tôhoku Mathematical Journal.*
9. Wash. Nat. Ac. Sc. Proc.: *Proceedings of the National Academy of Sciences.*

The following abbreviations will also be used:

1. An. J.: *Astronomical Journal.*
2. Ap. J.: *Astrophysical Journal.*

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T. C. Mendenhall

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XVI—SEVENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

THOMAS CORWIN MENDENHALL

1841–1924

BY

HENRY CREW

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1934

THOMAS CORWIN MENDENHALL

1841–1924

BY HENRY CREW

A sense of historical continuity is quite as essential to the advancement of science as to the growth of a nation. The man of science who has no memory for its past is not likely to be greatly concerned about its future. When Newton spoke of standing upon the shoulders of those who preceded him, he expressed a capital interest in the past. In some idea such as this doubtless lay the prime motive which led the early members of this Academy to institute a series of biographical sketches of its members.

The writing of such a sketch, however, is not easy. There is first of all the limitation of space imposed by rule, some fifteen or twenty pages. Besides this there is the difficulty of conveying to any, save a few, of one generation the relative importance of the work of the preceding generation. No man has ever appreciated Galileo more highly than did Newton; yet the experimental work of the great Italian must have impressed the Cambridge scholar as crude. *Mutatis mutandis*, the same might be said about Fresnel and Michelson; or about Rumford and Joule. Again, to tell the truth about an experiment in physics is child's play, compared with telling the truth about a man; for even a man of science is a social as well as a rational being; and his achievement in the increase of knowledge is not the whole of what makes him a valued member of this Academy. His uprightness, his courage, his generosity of mind, his geniality of spirit—or perhaps the want of it—are qualities which his successors deserve to know. Herein lies a second motive for these biographies. With these considerations in mind let us proceed to the life of our late colleague, Thomas Corwin Mendenhall.

ANCESTORS

His ancestry traces back to a family which came to this country as members of the Penn colony in 1686 and settled in Delaware county, in the extreme southeast corner of the state of

Pennsylvania, and in a village which was then called Concord, but which now enjoys the name of Concordville. In this same neighborhood, there is, at the present time, on the Maryland Division of the Pennsylvania railroad, a station named Mendenhall.

It is generally agreed that the earlier form of the family name was Mildenhall; but there is a curious uncertainty as to the exact location of the ancestral home; for it happens that in England there are *two* towns which carry the name of Mildenhall, one in the northwest corner of the county of Suffolk and another in the northeast corner of Wiltshire. In *American Ancestry, 8,* 60 (1893) one finds the definite statement that Benjamin Mendenhall came from Wiltshire, England, in 1686 and settled in Concord, Pennsylvania; while in George Smith's *History of Delaware County,* p. 484 (Philadelphia, 1862) one reads that "Benjamin Mendenhall came from England in 1686 probably in company with his brother, John. They came from a town of Suffolk called Mildenhall, that being the original family name." This latter view harmonizes with the following statement in the *Publications of the Genealogical Society of Pennsylvania, 4,* 249: "Benjamin Mendenhall came from a town in Suffolk, England, called Mildenhall in 1686. He settled in Concord." Henry Graham Ashmead in his *History of Delaware County,* p. 563 (Philadelphia, 1884) says "Three brothers, John, Benjamin, and George Mendenhall came from England with William Penn in 1682. George returned after a brief sojourn in America, John settled in Chester county and Benjamin found a home in Concord township in Delaware county. He was married to Ann Pennell and had sons, Robert and Benjamin."

Any doubt as to whether our Mendenhall family came from Wiltshire or from Suffolk is, I think, largely removed by the existence of a deed which conveys "500 acres of land in Penn's tract to Moses Mendenhall of Ramsbury, Wilts., date 1685, Sep't. 5th." A copy of this deed is given opposite p. 9 of a *History, Correspondence and Pedigrees of the Mendenhalls of England and the United States* by William Mendenhall, of Bath,

England, extended by his son Edward Mendenhall of Cincinnati, Ohio. (Cincinnati: Moore Wilstach & Baldwin, 1865.)

Charles Sumner is credited with the remark that he never passed a small boy on the street without feeling the urge to lift his hat, realizing the possibilities which are latent in the mind and body of such a lad. It is the absence of just such an urge, I fear, which has been responsible for the laxity of our habits and laws concerning the registration of births, marriages, and deaths. It is, in fact, the absence of just such a record concerning the Benjamin Mendenhall who came to America with the Penn colony which makes the first section of this sketch slightly ambiguous.

From here on, however, the story is perfectly clear. As indicated above, the Englishman, Benjamin Mendenhall, who came to America and joined Penn's colony had a son named Robert who was born in 1713 and who also lived in Concord township, Pennsylvania, where he died in 1785. This Robert Mendenhall, who during his entire life was a contemporary and near neighbor of Benjamin Franklin, had a son, named Stephen, who married Mary Farlow. To them was born a son whom they called James and who was the grandfather of the subject of this sketch. James Mendenhall was still living in southeastern Pennsylvania when his son Stephen, the father of our subject, was born in 1805. Shortly after this, in 1810, the family of James Mendenhall moved to what was then known as "the far west," Beaver county, Pennsylvania.

Left fatherless at an early age, this Stephen Mendenhall returned to the neighborhood of Philadelphia, where, a few years later, Mary Thomas became his wife. With her he journeyed again, in 1835, to "the far west," this time making a new home in Columbiana county, Ohio, just across the state line from Beaver county, Pennsylvania. To Stephen Mendenhall and his wife three sons and two daughters were born. The birth of the youngest son occurred on the 4th of October, 1841, while the family was still living at Hanoverton in Columbiana county. The little lad was named Thomas Corwin after a much beloved man, at that time Governor of Ohio, who was later appointed

Secretary of the Treasury and who still later was our Minister to Mexico.

YOUTH

Here, in the beautiful hills of eastern Ohio, the young Mendenhall spent the first twenty years of his life, the twenty years just preceding the first administration of Lincoln, twenty years in a Quaker family where the parents not only advocated, but also practiced, the simple life, frugality, sincerity, plainness of speech and dress, temperance, and the abolition of slavery.

The elder Mendenhall was a carriage-maker and had the pleasure of having his two older sons as partners. In addition to this business, he maintained a farm. Now the amount of knowledge concerning the properties of matter, the behavior of machines, and the processes of life which an eager and gifted boy picks up during twenty years of association with the lathe and work-bench of his father's carriage shop, in running the engine for his brother's steam saw mill, in assisting his mother in the churning of cream, in watching the mining of coal from the adjacent hills, in observing the building of the Cleveland and Pittsburgh railroad, is something amazing. All these and many more details are set forth in a story of his youth which Professor Mendenhall wrote, at odd times, during his years of rest, 1901-1911, spent in Europe. These *Recollections* of Professor Mendenhall, as I shall call them for the sake of easy reference, cover some 900 pages of manuscript and give a pleasing picture of abundance without overproduction, of economy as a fine art, of comfort and independence, and of the happy domestic life of the early Ohio settlers. The whole narrative is written in an easy—almost conversational—style and is, in my judgment, well worthy of independent publication.

Mendenhall's first-hand acquaintance with the world was supplemented by the usual primary school training of the period and by his own independent pursuit of mathematics, a subject to which he was ardently devoted. His well-known ability as an expositor was early adumbrated in his desire to teach school, an ambition which was first satisfied in the autumn of 1858 when he became assistant to the principal at Marlboro, in Stark county,

Ohio, a town to which the Mendenhall family had moved in 1852.

In the spring of 1859, he attended a teacher's institute at Alliance, Ohio, and there came under the brief instruction and influence of James A. Garfield, at that time president of Hiram College. The young teacher was much impressed by Garfield's definition of an adverb as "the skin of a verb stuffed with the bran of an adjective."

Much more powerful was the influence of Professor C. A. Young, the eminent and beloved astronomer, then teaching in Western Reserve College in Hudson, Ohio. Here Mendenhall had the rare privilege of studying with Young during the summer of 1864. Like all the rest of Young's students, Mendenhall felt about him much as Helmholtz did about his great teacher in physiology: "Once to have known Johannes Müller is forever after to be a different man."

The incident which brought to Mendenhall a brief period of study in an institution of collegiate rank is, at the same time, an excellent illustration of the power of the printed page. In the spring of 1861, he was looking over the shelves of a book-store in Cleveland, Ohio; and, having lighted upon a copy of Stoddard and Henkle's *Algebra,* he was so delighted with their novel mode of presentation that he then and there determined to pursue the subject under the immediate personal instruction of a man who could expound mathematics so clearly. The result was that he at once went to the Southwest Normal School at Lebanon, Warren county, Ohio—the home town of Tom Corwin—where W. D. Henkle was then teaching. Within a year he graduated with the degree "I. N.", *Instructor Normalis,* the only degree in course which he ever received. For the interpretation of this degree, "I. N.," I am indebted to the charming little biography of Mendenhall written by his friend Professor W. H. Siebert, of Ohio State University, as a part of a *History of the Columbus High School.*

The years between Mendenhall's leaving Lebanon and his election to the first faculty of Ohio State University in 1873 are crowded with important events in American history; but in the

story of the Mendenhall family the one outstanding fact is the marriage of Mr. T. C. Mendenhall to Miss Susan Allen Marple on July 12, 1870. Miss Marple was a descendant of Major Solomon Allen who carried Major André back to West Point after his capture; her home was in Columbus, and she was a student in the high school where her prospective husband was teaching. This union was the beginning of a long and devoted companionship, extending through the next forty-six years, that is, up to the time of Mrs. Mendenhall's death in 1916. On the first day of August, 1872, the new home was brightened by the birth of a son, Charles E. Mendenhall, well-known physicist and present member of this Academy.*

PROFESSIONAL LIFE

By this time the young high school teacher had proved himself to be an excellent demonstrator and an attractive public speaker. He had a rich tenor voice and that innate courtesy which comes from a consideration for the feelings of others. In his experience in various parts of the state, he had made the acquaintance and had acquired the respect of a number of its leading citizens. Moreover, he was able to qualify as a university professor under the definition laid down by President D. C. Gilman of Johns Hopkins University, namely "a student who can also teach." When, therefore, the Ohio Agricultural and Mechanical College—one of the first of the Morrill Land Grant institutions—opened its doors in 1873, it was in every way natural that this well-known teacher of science was appointed to the chair of physics and mechanics. Here, for the next five years, he went through the usual routine of a university professor at that time, attending also to various subduties, such as outside lectures, the chairmanship of certain committees, the general secretaryship of the American Association for the Advancement of Science, etc.

From the very start Mendenhall was one of the group who were in favor of taking advantage of the liberal provisions in the

* Died August, 1935.

Morrill Act and of making the institution liberal in the best sense of that word. The name of the college was shortly changed to Ohio State University, an institution recognized as one which, in the phrase of the Morrill Act, "promotes the liberal and practical education of the industrial classes * * * without excluding other scientific and classical studies."

Of the original faculty, the two leading men were doubtless Edward Orton and T. C. Mendenhall. Many years later, the latter of these two men made the following remarks concerning this first faculty:

"I shall avoid making comparisons of their work with that of their successors of today, for they belong to a different class. In many respects 1870 is to 1920 a period of almost indefinite remoteness. The college professors of today partake very largely of the nature of a manufactured article. They are highly specialized and accurately standardized. Most of the larger and some of the small institutions of learning are engaged in their production; and by some they are kept 'in stock' for the convenience of others. Today the chancellor of one great university may call by wireless telephone to the president of another something like this: 'Please send me at your earliest convenience one Ph. D. who has "majored" in this and "minored" in that, who has published not fewer than five original papers of not less than one thousand words each, and whose intelligence test is not below 155 nor above 200.'

"The first faculty of the University was not recruited in that way. In 1870 there were no such sources of supply, and the college professor of that period was more or less an accidental product; and yet, in one important sense, far less an accident than his successor of today, for in most cases a professorship was the goal of an ambition dating from early youth." *History of Ohio State University*, Vol. 3, p. 189.

RESIDENCE IN JAPAN

In the spring of 1878, the late Dr. Edward S. Morse returned to America during a brief interval between his visits to Japan and was stopping with his long-time friend Mendenhall in Columbus where he was lecturing upon Japanese homes and Japanese art. This distinguished naturalist carried with him from Japan what was practically a commission to appoint two Ameri-

can professors in the University of Tokio, one to the chair of philosophy, the other to the chair of physics. This commission he executed by recommending Professor E. F. Fenollosa in philosophy, and Professor T. C. Mendenhall in physics. Both recommendations were adopted by the Japanese government; and both men accepted appointment. The autumn of 1878 found Mendenhall, with his family, in Tokio where he was one of that outstanding group of western scholars who at that period were giving themselves unselfishly to the introduction of our mode of thought into the Orient.

The schedule of work, as first planned by the university, he voluntarily increased, giving public lectures by use of an interpreter, exhibiting the newly invented telephone to members of the nobility, and devoting much energy to the equipment of the laboratory of physics; for, as he remarks in his *Recollections,* "I was young, strong, and loved work." Many of his eager and able Japanese students became life-long friends, notably Kikuchi who interpreted his early lectures to popular audiences, and who later became Minister of Education, and still later, as Baron Kikuchi, Vice-Minister of the Navy. Tanakadate and Fujisawa made brilliant careers as students in European universities, and later by their original contributions in the field of physics.

Among the foreign contingent on the university staff at Tokio in those days, may be mentioned, in addition to Professors Morse and Fenollosa, the name of W. S. Chaplin, later Chancellor of Washington University at St. Louis, and the four well known English engineers, John Perry, William E. Ayrton, James A. (now Sir Alfred) Ewing, and John Milne.

Mendenhall's own investigations in Japan were mainly along the line of meteorology and geophysics. It was here that he determined the density of the earth by measuring the acceleration of gravity at sea-level and at the top of Fujiyama. Spectroscopy was then at a stage where accurate wavelengths in the solar spectrum were sorely needed. Mendenhall set about to supply these, employing the best apparatus available, namely a grating by Chapman and a spectrometer by Fauth, after the design of

C. A. Young. The results were published in a monograph by the University.

In the summer of 1880, a somewhat urgent request came from Dr. Edward Orton, then President of Ohio State University, asking Professor Mendenhall to return to Columbus and to devote himself entirely to physics, allowing Professor S. W. Robinson to take the new chair of mechanics. Home ties, old friends, and a charter-membership in the faculty of the state university were considerations too strong to be withstood. Accordingly the summer of 1881 found the Mendenhall family leaving Japan, but not without great reluctance. The following sentence from his *Recollections* will indicate something of the high regard which this professor of physics had acquired for his Japanese students; "Always fond of teaching and always enjoying my college work, I cannot but look upon my three years with these well-mannered, good tempered, ambitious, and intellectually strong men as being, in most respects, the pleasantest and best of all my professional years."

THE SECOND PERIOD AT COLUMBUS

Again in the chair of physics at Ohio State University with enlarged experience, widened horizon, and renewed vigor, Mendenhall gave himself generously to the routine duties of his office and to such extramural demands as the vice-presidency of the American Association for the Advancement of Science for Section B, a course of lectures before the Lowell Institute upon the Molecular Theory of Matter, a report on atmospheric electricity to the Electrical Conference of 1884 in Philadelphia, and various popular addresses. It was during this period that Mendenhall originated, and secured financial support for, the Ohio Meteorological Bureau, the purpose of which was to gather a knowledge of meteorological conditions, to forecast the weather, to exhibit weather indications in public places, and especially to carry them on the sides of the railway trains which sped across the farms of Ohio. These were the days when the applications of electrical science were taught and demonstrated only in laboratories of physics; the days when, to use the phrase of Mr. Rod-

erick Macrae, "Lord Kelvin was creating the science of electrical engineering while he was thinking of other things." Naturally, therefore, Professor Mendenhall was deeply interested in the electrical exhibits at the Cincinnati Expositions, and served on the jury there; and likewise at the Electrical Exposition held at Philadelphia in the autumn of 1884.

Activities of this type brought him a call, in December of 1884, to a professorship of electrical science in the U. S. Signal Corps at Washington, D. C. Earlier in the autumn of this same year, he was one of the "coefficients" who attended the twenty "conferences" held by Lord Kelvin *, then Sir William Thomson, at Johns Hopkins University.

Having by this time become widely acquainted with men of science in Washington, Mendenhall accepted the proffered appointment and soon found himself associated in office with Cleveland Abbe, William Ferrel, and his former student, C. F. Marvin. General A. W. Greeley was on the staff, but at this time absent in the Arctic regions. It was at this period and a little earlier that Mendenhall became well acquainted with and a close friend of Henry A. Rowland at Baltimore. As chief of the Instrument Division in the Signal Corps, it became his duty to visit Charleston, S. C., immediately after the great earthquake of Aug. 31, 1886; and he was thus led to establish the earliest stations in the United States for the observation of earthquakes.

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* Kelvin's *Baltimore Lectures* (Camb. Univ. Press: 1904) represent his final attempt to explain the phenomena of light in terms of an elastic solid theory. And since the names of his hearers—dubbed "co-efficients", in a humorous afterdinner poem, by the late Professor George Forbes—have not hitherto been published, so far as I am aware, it may be well to give here the following list furnished by Kelvin himself in a letter addressed to Johns Hopkins University under date of 14 January 1904: Lord Rayleigh, Henry A. Rowland, Eli W. Blake, Jr., Cleveland Abbe, Albert A. Michelson, Fabian Franklin, J. W. Moore, Diaroku Kikuchi, Arthur S. Hathaway, John R. Uhler, George Forbes, Henry Crew, John E. Davies, Thos. A. Smith, Louis Duncan, A. S. Kimball, John T. Hedrick, Clayton C. Hall, J. F. Arnold, Arthur L. Kimball, Christine Ladd Franklin, T. C. Mendenhall, J. M. Mansfield, Edwd. W. Morley, R. W. Prentiss, Gustav A. Liebig, Charles A. Perkins, R. W. Gatewood.

For his work in seismography, cartography and terrestrial gravity, he was awarded a gold medal at the Paris Exposition, 1900, and another by the American Geographical Society in 1901. Between office hours, time was found to prepare a small volume for the *Nature Series* which was then being issued by the Macmillan Company of London. The preface of the book, *A Century of Electricity,* is dated at Washington, D. C., May 16, 1886. As a piece of elementary exposition and as a clear historical recital of what happened in electrical science during the hundred years immediately following Galvani, the volume leaves little to be desired. It is, in fact, an excellent pre-Hertzian picture.

ROSE POLYTECHNIC INSTITUTE

During all the time Mendenhall spent in government duties, he never lost his high regard for the profession of teaching. In 1885, Ohio State University had made him an emeritus professor of physics; but no compliment of this kind ever slackens the activity of a man whose inspiration comes from within. Accordingly when a call came, in 1886, to the presidency of the three-year-old Rose Polytechnic Institute at Terre Haute, Indiana, he promptly accepted. Here he worked for three years, striving to place the education of engineers upon a solid foundation of training in the fundamental sciences, and to give the prospective engineer also linguistic and liberal studies sufficient to enable him to meet with ease his peers in the industrial, financial, and social world. Among his colleagues on the faculty here may be mentioned Professor Thomas Gray who held the chair of dynamic engineering, Professor W. A. Noyes, then at the head of the department of chemistry, now at the University of Illinois, and Professor James A. Wickersham, now emeritus professor of English, living in Terre Haute. It was at this period (1887) that Mendenhall was elected to membership in the National Academy of Sciences and, in the year following, to the presidency of the American Association for the Advancement of Science over whose sessions he presided at the Toronto meeting in 1889.

SECOND RESIDENCE IN WASHINGTON

Much indoor work had, however, begun to tell upon his hitherto robust health. Accordingly, when President Harrison's Secretary of the Treasury, Mr. Windom, offered him the superintendency of the U. S. Coast and Geodetic Survey in the summer of 1889, Mendenhall accepted, partly on account of his interest in geophysics and partly in the hope of more fresh air and sunshine. Again in Washington, many new acquaintances were added to those above mentioned. Fast friendships were cemented with S. P. Langley, J. W. Powell, Gardiner Hubbard, Graham Bell, Simon Newcomb, F. W. Clarke, and L. O. Howard.

The Bureau of Standards had not yet been created. "Weights and Measures" formed a subdivision of the Coast and Geodetic Survey; and the superintendency of this Survey was, with the possible exception of the directorship of the U. S. Geological Survey, perhaps the most important scientific appointment in Washington.

The multiplicity of duties connected with this office may be indicated by mentioning that within the next five years Mendenhall was engaged in a relative gravimetric survey, using the portable short pendulum of his own design; was off on a trip to Alaska in the interests of a boundary settlement; was a member of the first Behring Sea Commission; was a member of the U. S. Light House Board; and was an "official delegate" to the International Electrical Congress held in Chicago in 1893. In addition to these governmental duties, he found himself appointed on five different committees of the National Academy of Sciences; and in 1892 gave a second series of Lowell Lectures on "Measuring the Earth."

The interest attaching to the International Electrical Congress of 1893 arises largely from the fact that this was the assembly which gave to the so called "practical units" of electrical science their present form and meaning. It was here also that the last of these eight units—that of inductance—received its name, the *henry*. The legislative chamber, which was charged with decision as to units, names of units, and standards, was composed

of 26 members representing the more important nations of Europe and North America. Among others representing Europe, were Helmholtz, O. Lummer, E. Mascart, T. Violle, S. P. Thompson, W. H. Preece, W. E. Ayrton, and Galileo Ferraris. The American delegates were H. A. Rowland (President of the Chamber), T. C. Mendenhall, H. S. Carhart, Elihu Thomson and E. L. Nichols. Owing largely to national prejudices, the sessions were prolonged through an entire week in August. Many differences arose as to what were the essentials and what were the frills in the various phrasings of these eight definitions. The form finally adopted for the three basic units, *ohm, ampere,* and *volt,* and soon afterwards approved by the various national governments, [*Proc. Internat. Elec. Congress.* Chicago, 1893, p. 20. (1894)] was however a draft which Mendenhall had worked out, in the privacy of his own room, the evening before. The original of this protocol of Mendenhall's, to which Rowland, Helmholtz, Mascart and Ayrton had, in advance of its adoption, affixed their signatures is now in possession of the Franklin Institute. These details are introduced as evidence of Mendenhall's mastery of clear thought and concise English.

WORCESTER POLYTECHNIC INSTITUTE

President Cleveland, during his second term, placed no less than 44,000 appointments on the classified list of the civil service, thus bringing them under the merit system; but this did not prevent some of the members of his cabinet from beginning the redistribution of some of the better offices, even in the scientific bureaus, immediately after the inauguration, March 4, 1893. Opposed on principle to this feature of democracy, Mendenhall felt such appointments and dismissals, thrust upon the Coast Survey from the outside, to be an indignity. He still cherished his old love for the profession of teaching and accordingly resigned his Washington post in the spring of 1894 in order to accept the presidency of Worcester Polytechnic Institute in Massachusetts. In making this move Dr. Mendenhall entered his protest against the appointment of men to governmental posi-

tions of technical importance merely as a political reward and without any desire to improve the service.

The summer of 1894 was spent abroad. Then came seven years of educational and administrative work in which the effort was, with the aid of an able faculty, to secure the same high ends which were sought at the Rose Polytechnic Institute. And here again many extramural duties thrust themselves upon him. Chief among these was perhaps the chairmanship of the Massachusetts Highway Commission, whose problems, following the introduction of the automobile, were just then assuming a new and first-rate importance. It was about this time, 1895, that the new series of *Science* was begun. On the editorial committee, physics was represented by Dr. Mendenhall. In this same year, 1895, he became a member of the Society for the Promotion of Engineering Education; and so helpful and influential was he in the Council of that body that, in 1911, the Society elected him to honorary membership, the only distinction of the kind it had then granted.

EUROPEAN RESIDENCE

In 1901, President Mendenhall celebrated his sixtieth birthday anniversary, resigned his presidential office, and went to Europe, with Mrs. Mendenhall, in search of rest, recreation and health. This trip proved to be a very long one, covering ten years. The time was divided between the Azores, Geneva, Lucerne, Rome, Florence, Lugano and other parts of Italy. Dresden, Egypt, India and Japan were also visited. The warm—even affectionate—reception which the Mendenhalls received, in 1911, during their three months' stay in Japan was a grateful reward for services performed thirty years earlier. Any student of modern Japanese history will be well repaid in reading Mendenhall's excellent picture of the almost incredible changes which took place in Japan within this brief third of a century. His paper will be found in the *Journal of Race Development*, 2, 224-235, (January, 1912).

BACK TO OHIO

The last step in this long odyssey of eleven years brought the travellers, in 1912, to Ravenna, the principal town of Portage County, Ohio, in a neighborhood where relatives and old friends awaited them. Marlboro, the boyhood home of the Mendenhalls was only seventeen miles away: Cleveland only thirty in the opposite direction. Columbus was within easy reach. Ravenna offered much to a gentleman of leisure and a man of cultivated tastes. Here was a commodious home, genial friends, good books, wide interests, peace of mind, *otium cum dignitate*. Bereft, in 1916, of his devoted wife, the inspiring woman with whom he had travelled more than forty years of life and the mother of his worthy son, he was left much alone, yet with a host of priceless memories. Letters of sympathy came from many parts of the world.

Within the next few years, the Franklin Institute bestowed upon him the highest honor in its gift, the Franklin Medal. It was upon this occasion that he gave some of his *Metrological Memories* in the form of a highly interesting address. Here he relates just how, on April 5, 1893, in a very quiet, but thoroughly open, manner the United States standards of length and mass were shifted from the English yard and pound to the International Meter and Kilogram.

No one can follow the details of this man's life without early discovering his deep and lasting affection for Ohio State University. It was in this seat of learning that he first realized the ruling passion of his youth. Here he met his students a little more intimately than elsewhere. Members of his classes, among whom I may mention Mrs. W. H. Siebert and Professor Charles F. Scott, tell me that he never gave a lecture which did not challenge the interest of the hearer or one which was not, at some point, enlivened by humor. Here he gave his best, without money and without price. It was therefore a matter of deep satisfaction to him and to a host of his former students that Governor James M. Cox appointed him to the Board of Trustees of this university, a body which shortly elected him to its chairmanship. The new building devoted to physics was ap-

propriately named the "Mendenhall Laboratory." One of his last and characteristic contributions to this university was the endowment of the Joseph Sullivant Medal (named in honor of an old friend), to be given once in every five years for an admittedly notable achievement by a son or daughter of the university.

His death occurred at Ravenna on March 22, 1924.

This brief and inadequate sketch may perhaps be brought to a close by a single paragraph from a universally beloved and lifelong friend of the man and of the university. In a letter to me under date of February 16, 1934, President Emeritus William H. Scott writes: "I knew Professor T. C. Mendenhall well before I became associated with him as a member of the same faculty, having met him at State Teacher's Associations, at a county association of which he was one of the instructors, and in my own home. As a teacher he was one of the two or three best I have known—clear, simple, attractive, and impressive. As a lecturer he excelled and was very popular. As a member of the faculty, he did his part with an eye to the whole as well as to his own department. Few men whom I have known had so many friends and admirers."

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(i) A definite and easily observable length to measure, namely, the external diameter of the ring.

(ii) The great rigidity of the pendulum; hence but slight departure from the measured figure when suspended.

(iii) Detection of, and partial correction for, non-homogeneity of the pendulum.

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(i) $g = 980.526$
(ii) $g = 980.511$

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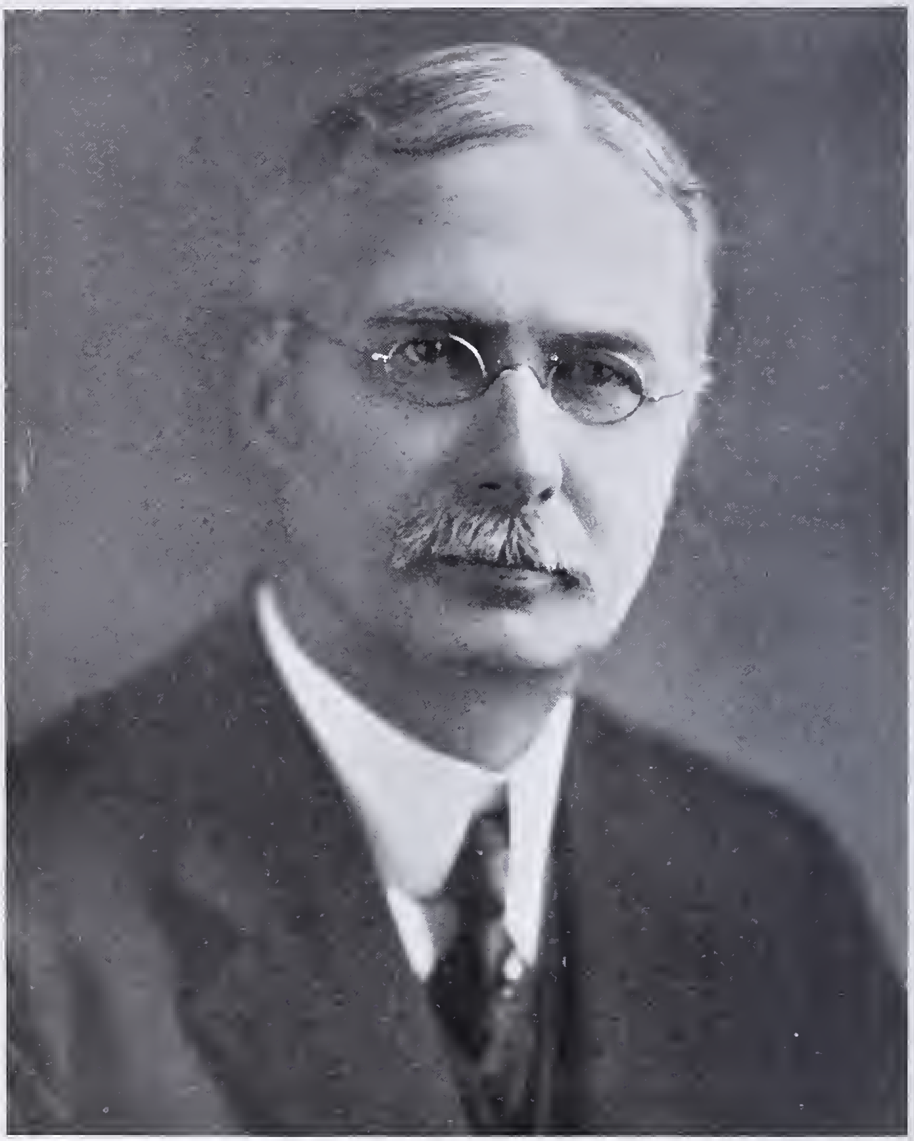

E. B. Rosa

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XVI—EIGHTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

EDWARD BENNETT ROSA

1861–1921

BY

W. W. COBLENTZ

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1934

EDWARD BENNETT ROSA

1861–1921

BY W. W. COBLENTZ

Edward Bennett Rosa, physicist, was born at Rogersville, Steuben County, N. Y., October 4, 1861, and died suddenly while engaged in work in his office at the National Bureau of Standards at Washington, D. C., in the afternoon of May 17, 1921.

He was the son of Reverend Edward David and Sarah Gilmore (Roland) Rosa; the grandson of Cornelius and Mary (Doty) Rosa; and a descendant of Albert Heymans Roosa, who emigrated from Holland in 1660 and settled with his wife and family of eight children on the Hudson River, near Newburgh, N. Y.

He was married to Mary Evans, daughter of William W. Evans of Harrisburg, Pa., on March 22, 1894. There were no children.

As a son of a Methodist clergyman one can picture Rosa's boyhood days amid religious and educational surroundings that left an impress throughout his subsequent years. His college education was obtained at Wesleyan University, Middletown, Conn., from which he was graduated at the head of his class, receiving the degree of B. S. in 1886.

After leaving Wesleyan University he taught physics and chemistry in the English and Classical School in Providence, R. I., where he remained two years. He then entered Johns Hopkins University as a graduate student in physics (under Henry A. Rowland, elected to the Academy in 1881), and received the degree of Doctor of Philosophy in 1891. In 1906, in recognition of his contributions to science, the honorary degree of Doctor of Science was conferred upon him by Wesleyan University.

During the first part of the year 1890 Dr. Rosa was assistant professor of physics at the University of Wisconsin, leaving there to become associate professor of physics at Wesleyan Uni-

versity, in 1891, and professor of physics in 1892. He retained the professorship of physics (the Charlotte Augusta Ayers' professorship) for ten years, when, in 1901, he was called to the newly-organized National Bureau of Standards, at Washington. There, as physicist, and later on, as chief physicist, he continued through the remainder of his life. His was a short span of three score years—one score of which was spent at the National Bureau of Standards.

In stature Dr. Rosa was tall, well built, of distinguished and healthy appearance. His life out of doors was abbreviated to tennis playing. Its sudden termination was the result of a cardiac disturbance of short duration. The signature under his portrait, taken from an official report, was selected by one of Rosa's former colleagues, as being representative of his style when he was "feeling fine," and not overcrowded with work. But even then he was fairly deliberate in thought and action.

Dr. Rosa's interests in science and his outlook upon life were broad. While he was not of the jovial type he was not without a sense of humor. The writer recalls a staff meeting at which a fellow member described a complex electrical device, said to be capable of a wide range of uses. It was provided with a series of automatic blocking switches to make it "fool proof" from accidents. So much emphasis was placed upon the "fool proof" feature that Rosa finally interrupted the presentation with the comment that "fools should not be allowed to work with it." This remark represents more than humor. It represents Dr. Rosa's administration of his division of the Bureau with the best of equipment and the best of assistance to conduct a magnificent program of work, some of which was in competition with similar, older foreign institutions.

Dr. Rosa was fully conscious of the possible narrowing influence of high specialization, such as obtains in the National Bureau of Standards, and at a meeting of the Bureau's Physics Club (devoted to a general review of scientific papers), he once digressed from the topic under discussion, to emphasize that an inevitable consequence of high specialization is that "we grow taller and thinner." To the writer this condition seems prefer-

able to desiccation, or "flattening out" as a result of attempting to "broaden out"—a common failing. However, in the National Bureau of Standards, which owes part of its high standing to Dr. Rosa and to which Rosa, in return, was indebted for the opportunity to develop his latent abilities, the percentage of "flats" is relatively small.

In his personal relations with the other large subdivisions of the Bureau, Rosa was very human, guarding jealously the interests and accomplishments of his own division, yet withal proud and fair in his appraisal of the accomplishments of other divisions. The writer has reason to know this to be the case. For in his search for new thermoelectric material, and in his development of bolometers and of magnetically highly shielded Thomson galvanometers (all electrical instruments), "for use in radiometry," the writer was constantly overstepping the imaginary boundary between the Optical Division and the Electrical Division of the Bureau. While this sometimes appeared to be disappointing to Dr. Rosa, nevertheless he was evidently pleased with the development of these instruments, particularly the galvanometer; for he brought it to the attention of the members of his staff, who met at his home one evening for a discussion of work in his division.

Dr. Rosa's research work began at Wesleyan University, where in association with Professor Wilbur O. Atwater, he developed the physical side of the respiration calorimeter, known under the joint name Atwater-Rosa respiration calorimeter. The practical details of the construction of the instrument were chiefly Dr. Rosa's. This apparatus was of great value in the pioneer investigations of the value of foods, and in the study of problems in nutrition.

While at Wesleyan University he invented and developed a curve tracer (the Rosa curve tracer) for delineating the form of alternating electric currents, a problem of interest in the operation of alternating current machinery. The original curve tracer is still to be seen in the physical laboratory of Wesleyan University.

Probably the most important epoch in Dr. Rosa's scientific life began in 1901 when he undertook work at the National Bureau of Standards, under the directorship of Dr. Samuel W. Stratton (elected to the Academy in 1917). At that time the major divisions in the Bureau were: I, Electrical; II, Weights and Measures; III, Heat; IV, Optics; V, Chemistry—each subdivided into sections, which increased in number with the complexity of the work involved.

In those days, the second in command was Dr. Rosa, ranking physicist and chief of the electrical division, where from the start he proved his abilities as an efficient administrator. He kept in intimate contact with each section, and, in collaboration with his section chiefs and their assistants, he conducted researches; notably in photometry, inductance, capacitance, etc., as evidenced by the appended bibliography of published papers. While this did not "make men," it unquestionably was the best arrangement for the accurate determination of the fundamental electrical constants, which required the mature judgment of all who were engaged in that work.

When Dr. Rosa began his work in the Electrical Division of the National Bureau of Standards it was his ambition to determine a number of the fundamental electrical constants to a degree of accuracy far exceeding all previous determinations. To partly attain this goal he was singularly fortunate in having as a co-worker, Dr. N. E. Dorsey.

One of these determinations was the ratio of the electromagnetic and the electrostatic units. This work was started early in 1907 in conjunction with Dr. Dorsey, through whose skillful and painstaking experimental technique there resulted the most accurate determination yet made of this constant.

About 1907 Dr. Rosa with Dr. Dorsey started their determination of the absolute value of the ampere. This work extended over a period of years, and gave a more reliable value of the ampere than any previously obtained. In order to obtain a concrete representation of the ampere, Dr. Rosa with the assistance of Dr. G. W. Vinal carried on an investigation of the silver voltameter simultaneously with the absolute determina-

tion of the ampere, and it is largely as a result of this work that we are now able to define the ampere in a satisfactory manner.

Dr. Rosa served as secretary of the International Technical Committee on Electrical Units and Standards. In order to attain a better understanding of the methods used, and a better agreement in the results, he was instrumental in procuring an interchange of workers in the three national standardizing laboratories—Great Britain, France and Germany.

In the accompanying photograph is shown an informal gathering of part of the International Technical Committee on Electrical Units and Standards, (left, Dr. (now Sir) Frank E. Smith of the National Physical Laboratory of Great Britain; center, Prof. F. Laporte, of the Laboratoire Central d'Electricité; and right, Dr. Rosa) taken at the National Bureau of Standards in the spring of 1910, when this committee was engaged in an intercomparison of the silver voltameters, standard voltaic cells and standard resistances (in use in their respective countries) with those at the Bureau of Standards.

This interchange of workers, apparatus (standard incandescent lamps as standards of the luminous intensity, standard voltaic cells, and standard resistances, etc.,) and of ideas, has been of inestimable value in establishing the electrical and other units upon a high plane of accuracy.

About this time the problem of electrical capacity and inductance occupied Rosa's attention. He devised methods for measuring these quantities, and, in some cases, with the assistance of Dr. Louis Cohen, made calculations of the electrical characteristics of coaxial coils (Bibliography papers 60 to 66). The absolute measurements of inductance and capacity were made with Dr. F. W. Grover (papers 53 to 56). The final contribution consisted in collecting all the known formulas for computing inductance, which collection is reported to be in use the world over.

During this formative period of the National Bureau of Standards, Dr. Rosa contributed considerably to the establishment of units and a standard nomenclature in photometry. With E. C. Crittenden and A. H. Taylor he conducted researches on

the flame standards. He took a leading part in securing international agreement on a standard of luminous intensity, maintained by intercomparison of a series of incandescent lamps that are frequently interchanged among the national standardizing laboratories.

Looking back over all these years of activity in the National Bureau of Standards, it would appear that almost everything was happening in the second epoch of its history, beginning in the spring of 1905, when the Bureau was moved from its temporary quarters, located in old dwellings down town, into its new quarters in the then open fields, in the suburbs of Washington.

One of the "new things" was the observation by Dr. Dorsey that the value of the standard resistances, used by him in the determinations of electrical constants then in progress, underwent a seasonal change, which subsequently was found to depend upon the humidity. The writer vividly recalls Rosa's mental perplexity in describing the phenomenon as they rode to the Bureau—by street car, the mode of transportation in those days. The standard resistance boxes then in use consisted of coils of wires wound on wooden spools, covered with shellac, and sometimes paraffin. The shellac, being hygroscopic, evidently changed the tension on the wires (with change in humidity), and hence the resistance, by a sufficient amount to be detected by the observant Dorsey. Characteristic of his thoroughness, Dr. Rosa promptly started an investigation of this question (see Bibliography paper No. 69), and brought out a new design of resistance coil, wound on a metal form and sealed in kerosene oil, which became the model for subsequent standard resistances.

Dr. Rosa devised a new apparatus for determining the absolute value of the ohm. Models of this apparatus, which were tried in 1908 and 1909, gave promise of satisfactory results. However, the pressure of other work compelled the abandonment of this project; though he always hoped the time would soon come when it could be continued.

In 1910 under Dr. Rosa's direction an exhaustive investigation was instituted into the subject of electrolytic corrosion of

underground gas and water pipes, and lead cable sheaths, due to stray currents from electric railways. This problem has for years been one of major importance to public utility companies throughout the country, and prior to the work taken up under Dr. Rosa's direction at the National Bureau of Standards very little definite information was available as to the laws governing electrolytic corrosion or the methods of mitigating corrosion from this source. The work done under his direction included a definite establishment of laws governing electrolytic corrosion, and much progress was made in mitigating trouble of this nature. This work has for a number of years been carried on in close cooperation with the utility interests of the country through the medium of the American Committee on Electrolysis, of which Dr. Rosa was a member.

During the War, Dr. Rosa directed the development of a number of scientific instruments which were of great value to the American Forces in France. Among these were a sound ranging device for locating big guns; the geophone for the detection of mining operations; the development of aircraft radio apparatus; and the improvement of radio direction finders by which enemy ships and air craft could be located.

Under his direction at the National Bureau of Standards was established perhaps the finest radio research laboratory in the country, and he always showed an intense interest in improving apparatus and methods of radio communication.

In addition to his diversified work in the field of electrical research, Dr. Rosa was keenly interested in the prevention of industrial accidents and in the promulgation of safety standards for use by state, municipal and insurance organizations. He conceived the idea of a National Electrical Safety Code, and the present code is largely the result of his efforts. Similarly the Bureau undertook a number of other national safety codes, the Safety Code Section working under his direction.

His broad vision showed him the need of a central clearing house for engineering standards. For years he worked wholeheartedly to bring about the formation of such an organization.

It was due in no small measure to his efforts that the American Engineering Standards Committee is now functioning.

One of the popular diversions is the repeated portrayal of employes (Federal, state, and city) as a clamoring group of mendicants, ever ready to receive the semi-monthly dole of salary and ever ready to dodge service. The unfairness of the criticisms must have irked Dr. Rosa, as it has others. He alluded to it in his analysis of the employment policy of the Federal Government, in an address entitled "Civil Service Reform—a Reorganized Civil Service," before the Washington Academy of Sciences on October 23, 1920, and in his analysis of "Expenditures and Revenues of the Federal Government," presented before the American Academy of Political and Social Science, May, 1921. At the time they were issued these papers were quoted by leading periodicals, as well as in both Houses of Congress; and even after a lapse of almost fifteen years they are still commended for their accuracy and their freedom from political bias. While this sketch is being written the writer has before him a letter, dated July, 1934, in which the author (a civil service officer) expresses his surprise as to "how much of the (Rosa) article has a vital bearing upon present civil service problems."

According to one of the writer's informants, the analysis of government expenditures and revenues came about soon after the world war, when Rosa advocated, before the Congressional Appropriations Committee, the expenditure of more money on research, and was told that it would bankrupt the government. Characteristic of the man, Rosa then presented his charts, showing that, of the total net expenditures for the fiscal year 1920, amounting to $5,687,712,848, only $57,368,774 (1.01 per cent!!!) was expended on research, education and development, as compared with 23.7 per cent on the army and navy, and almost 70 per cent on obligations arising from previous and recent wars. In making these comparisons, which appeared somewhat invidious, Dr. Rosa distinctly emphasized his belief in adequate military preparedness. His analysis showed that in 1920, of the total of $53.46 per capita revenue collected through taxa-

tion, only 54 cents was spent for research, education and development; and he was led to wonder whether, instead of this 54 cents per capita, "if twice as much had been spent, it would not have made the burden of taxation lighter instead of heavier, by rendering a greater service to the people and creating wealth and aiding industry in larger measure."

In 1900 the Eliott Cresson Medal of the Franklin Institute was bestowed upon him in recognition of his work with the respiration calorimeter. That he did not receive more recognition is noticeable; but in these days, with the newspapers filled with pictures of people receiving trophies and medals for every conceivable achievement, however trivial, such recognition would probably have meant but little in Rosa's busy life.

Dr. Rosa was a charter member and one of the officers of the Federal Club, an organization of executives of the various governmental departments. He was a Fellow of the American Institute of Electrical Engineers, the American Philosophical Society, the American Physical Society, the American Association for the Advancement of Science (Secretary, Section B, 1898; Vice President, 1910); and a member of the National Academy of Sciences (elected in 1913), the Illuminating Engineering Society, the Philosophical Society of Washington (vice-president, 1907-12; president, 1912); the Washington Academy of Sciences, the American Engineering Standards Committee, and the (secretary) International Committee on Electrical Units and Standards. He was a member also of the Cosmos Club of Washington, and the Delta Kappa Epsilon Fraternity.

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